Competitive Effects of State Antitrust Laws: Evidence from the Progressive Era

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Abstract

Most U.S. states adopted an antitrust statute in the late nineteenth or early twentieth century to regulate anticompetitive conduct and promote competition among firms. However, despite states' pioneering role in the development of antitrust policy, little is known about the economic consequences of their efforts. I estimate the long-term effects of state antitrust laws on manufacturing outcomes using newly digitized state-by-industry data from historical censuses of manufactures. Using difference-in-differences and event study models that account for the staggered nature of adoption, I find that these statutes did little to limit the influence of powerful incumbents. The number of manufacturing establishments rose by about 10 percent following adoption, but evidence on ownership structure suggests that this growth was likely driven by the expansion of incumbent firms rather than new entry. I also find that the labor share declined by about 4 percent overall and by about 7 percent in trust-affiliated industries, suggesting that dominant firms held on to their market power despite new legal constraints on anticompetitive conduct. I find no evidence of increased employment or reduced profits, providing further evidence that firms made few meaningful adjustments to pricing or production behavior in response to these laws. These results suggest that antitrust legislation alone is insufficient to disrupt concentrated market power.

JEL classification: K21, L40, L60, N11, N41, N61, N81
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1 Introduction

Over the last four decades, the U.S. economy has seen rising concentration. Several studies, drawing on a variety of data sources and methodologies, document this pattern across a broad range of industries (Bajgar et al. 2019; De Loecker, Eeckhout, and Unger 2020; Grullon, Larkin, and Michaely 2019; Philippon 2019; Smith and Ocampo 2025; Zeballos, Dong, and Islamaj 2023). Research also links higher concentration to a declining share of income going to labor (Aghion et al. 2023; Autor et al. 2020; Barkai 2020). Together, these observations have spurred concerns about the possibility of reduced competition and increased market power in the United States.[1]

In light of these trends, and amid broad public support for stronger oversight, some have called for reforms to the nation's antitrust policies.[2] Antitrust laws restrict firms' ability to monopolize an industry or collude with each other and are designed to protect competition among firms. In turn, competition among firms can create benefits for consumers, such as downward pressure on prices and incentives for firms to maintain product quality. However, the empirical literature offers mixed evidence on the effectiveness of antitrust interventions in promoting competition. Consequently, whether more aggressive enforcement would reverse the increase in concentration observed in recent decades is unclear. To shed light on this question, I analyze a natural experiment in which 39 U.S. states enacted antitrust statutes over the course of the late nineteenth and early twentieth centuries. In particular, I examine the effects of these laws on competition and innovation, which allows me to offer evidence that is relevant to modern debates about the role of antitrust policy.

The origins of U.S. antitrust law lie at the state level of government. Thirteen states had already adopted an antitrust statute of their own by the time the Sherman Act—the first federal antitrust law—was enacted in 1890. Although federal antitrust law is often regarded as paramount, state antitrust statutes remained important well after the Sherman Act's passage. In fact, most states enacted their antitrust statutes after 1890, underscoring that state law was seen as a necessary complement to federal legislation, not a redundancy. The enactment of these statutes and the enforcement actions that followed often generated substantial media coverage, reinforcing the perception that state legislatures were leading the fight against trusts.[3] That perception was also

[1] There is active discussion over the extent to which these trends truly indicate greater market power. See, for example, Miller (2025), Shapiro and Yurukoglu (2024), and Werden and Froeb (2018).

[2] Support for reform is reflected in proposals from prominent scholars and politicians as well as in survey data on economists' and the public's views. For examples of calls to strengthen antitrust laws and enforcement, see Baker et al. (2020), Hawley (2025), Khan (2017), Warren (2024), and Wu (2018). Likewise, in a recent poll of leading economists, a majority of those surveyed agreed that the dominance of large technology firms merits either new regulatory measures or substantial reforms to antitrust policy (Kent A. Clark Center for Global Markets 2020). There is also strong support for regulation among the general public. In one recent poll, 68 percent of respondents said they support antitrust laws, and pluralities said stricter enforcement would benefit consumers, workers, small businesses, and the U.S. economy as a whole (YouGov 2023).

[3] For example, California newspapers described that state's law as "perhaps the most far reaching in effect of any of the laws of the legislature" (*San Francisco Call*, May 24, 1907) and reported that "trusts [went] 'bust' in just one week" under the new antitrust law (*Los Angeles Times*, May 16, 1907). Other coverage emphasized the expansive nature of state statutes, with the *Owosso Times* noting that Michigan's antitrust law was "sweeping in its nature"

grounded in a steady stream of legal activity at the state level. State attorneys general brought numerous cases, including actions against large interstate firms; courts often interpreted state statutes expansively; and penalties in some cases were severe (Bringhurst 1979; Lamb 2001; May 1987, 1990). Moreover, state courts were frequently the primary venues for addressing anticompetitive conduct in manufacturing and other key sectors until the New Deal expansion of federal commerce power (Columbia Law Review 1961). This evidence indicates that state antitrust laws had the potential to influence economic outcomes, prompting the question of whether they actually did.

I answer this question using data from four main sources. The first source pertains to state antitrust laws themselves. To identify state antitrust statutes in force in the late nineteenth and early twentieth centuries, I reviewed state session laws and legal codes from 1860 through 1940 and identified provisions restricting anticompetitive conduct, such as price fixing. After identifying all such provisions, I coded their contents to capture their main features. The second data source relates to manufacturing. To assess the impact of state antitrust laws on the manufacturing sector, I use newly digitized state-by-industry tabulations from the census of manufactures for 1850 through 1940 (Barkai, Karger, and Schaller 2025). The main outcomes I examine are the number of manufacturing establishments, the profits and labor demand of these establishments, and the labor share. The manufacturing sector comprised a relatively large share of the U.S. economy during the study period, which motivates my focus on manufacturing in this paper.[4] Third, I use data from the Comprehensive Universe of U.S. Patents (CUSP) to assess the impact of state antitrust laws on innovation, as proxied by patenting behavior (Berkes 2018). Finally, to identify newspaper articles mentioning one or more state antitrust laws, I searched two popular digital archives for terms relating to state antitrust laws. These articles allow me to understand how state antitrust laws were perceived by the general public and enforced by state governments during the study period.

I use "stacked" difference-in-differences models, which account for the staggered nature of treatment timing, to study the effects of state antitrust laws. This method involves stacking cohorts of states by adoption date and estimating treatment effects relative to untreated states, thereby avoiding biases common in conventional two-way fixed effects specifications. As is typical in difference-in-differences models, this approach exploits variation in the timing of states' adoption of antitrust laws and compares outcomes in states with antitrust laws to outcomes in states without them, before and after adoption. Because the timing of effects is of interest, I also employ event study models in my analysis. A key identification advantage of studying antitrust in a historical context is the presence of both a period without any antitrust laws and a clear control group of states that never adopted antitrust statutes, which papers studying antitrust in modern settings typically lack.

(July 19, 1889), the *New York Times* reporting Kentucky indictments against more than forty companies for "banding together and fixing" prices (March 23, 1899), and the *Iowa Plain Dealer* announcing that Missouri had revoked the charters of "about seven hundred trusts" for violating the state's antitrust law (November 23, 1889). These kinds of reports conveyed to readers that state antitrust laws represented a dramatic and far-reaching assault on big business.

[4]Figure A1 shows how the manufacturing sector's share of GDP evolved between 1850 and 2023. In 1890, shortly after the first state antitrust laws took effect, manufacturing comprised about 27 percent of nominal GDP.

My results suggest that, for all the attention surrounding their enactment, state antitrust laws fell short of meaningfully constraining powerful incumbents. I find that these statutes increased the number of manufacturing establishments by roughly 10 percent, but this growth was likely driven by the expansion of incumbent firms rather than new entry. At the same time, the labor share declined by 4 percent overall and 7 percent in trust-affiliated industries, while profits and employment remained unchanged, indicating that dominant firms fortified their positions despite new legal constraints on anticompetitive conduct. I fail to find systematic evidence that greater enforcement intensity or statutory stringency mitigated these effects. I also document an increase in patenting, pointing to positive spillovers on innovation and supporting the idea that concentration and innovation often coincide. These findings suggest that while state antitrust laws encouraged innovation, they did not meaningfully curtail the market power of dominant firms.

This paper is the first quasi-experimental, quantitative study to examine the effects of state antitrust laws around the time of their enactment. In doing so, I engage with several strands of literature. Among these, I speak to longstanding debates on the effectiveness of antitrust policy, where existing studies report mixed effects on competitive outcomes in both historical and modern contexts (e.g., Babina et al. 2023; Bosch and Eckard 1991; Burns 1977; Sproul 1993; Stigler 1966). I also inform discourse on antitrust and innovation (e.g., Kang 2025; Watzinger et al. 2020) and on collusion and firm behavior in historical U.S. settings (e.g., Alexander 1994; Gross 2020; Porter 1983; Vickers and Ziebarth 2014). Finally, I extend a body of work by economic and legal historians on the role of states in shaping competition during the late nineteenth and early twentieth centuries (e.g., Lamoreaux 2023; Lamoreaux and Phillips-Sawyer 2021; May 1987; Troesken 2000).

My central contributions to these literatures are fourfold. First, unlike studies that focus on incremental policy changes or individual lawsuits, my setting allows me to estimate the overall impact of a new antitrust regime, including potential deterrence effects. Antitrust law can affect firms' outcomes through both direct channels, which fall on firms subject to enforcement actions, and indirect channels, which operate through deterrence: even firms not targeted may alter behavior in anticipation of scrutiny.[5] Second, I broaden the scope of prior scholarship by studying the long-term effects of antitrust, employing data covering nearly a century, and examining effects on the entire manufacturing sector rather than focusing on individual industries. Third, I highlight the formative role of state competition policy, which has received less attention in the literature than the federal antitrust regime despite the fact that states pioneered antitrust regulation in the United States. Fourth, this paper advances our understanding of the Progressive Era, a period in American history characterized by sweeping reform. As society undergoes what some are calling a second Gilded Age,[6] lessons from this period provide valuable insight into the challenges of today's world.

[5]The ability to capture deterrence is a unique advantage of my setting. By discouraging firms from engaging in anticompetitive conduct in the first place, antitrust laws can influence not only the firms in the courtroom but also the broader economy. However, because most antitrust research focuses on individual lawsuits, estimated effects typically concern the parties to a case or their industries, while the broader consequences are harder to observe.

[6]See, for example, Wu (2018).

2 Background

2.1 Historical Context

The antitrust movement emerged during a period of rising concentration, as large firms increasingly displaced smaller competitors in the U.S. economy. As shown in Figure 1, references to "trusts" rose sharply in the mid- to late 1880s, underscoring the growing public attention directed toward big business.[7] This public concern emerged alongside major changes in transportation, communications, and other technologies that encouraged consolidation and eroded the position of small firms (Atack 1985; Higgs 1971; James 1983). Railroads, which made long-distance shipping easier and cheaper, allowed firms to serve increasingly distant markets (Donaldson and Hornbeck 2016; Hornbeck and Rotemberg 2019, 2024). New communications technologies, such as the telegraph (and later, the telephone), allowed national markets to emerge as firms could more easily do business with partners in far-flung places (Yates 1986). New production technologies, such as the Bessemer method of steel-making, favored mass production (Rogers 2009). The efficiencies associated with these technological advances enabled firms to dramatically increase output, often surpassing demand and resulting in widespread industrial overcapacity that pushed firms toward consolidation as a strategy for stabilizing prices and surviving competition (McCraw 1981). As a result, large firms began to capture increasing market shares; as Figure 2 illustrates, concentration in the manufacturing sector rose steadily in the decades preceding the antitrust movement. As large firms became more numerous, concerns about their growing power set the stage for the rise of the antitrust movement.

The relative decline of agriculture also played a role in the rise of antitrust. According to the standard "agrarian discontent" hypothesis, farmers found themselves increasingly vulnerable to the power of large corporations as the United States industrialized, leading them to lend strong support for antitrust legislation (Thorelli 1955, p. 58).[8] The geographic pattern of early antitrust legislation

[7] "Trust" became a catch-all term for industrial combinations perceived as threatening competition during the late nineteenth century (Werden 2020, pp. 11–17). Originally, however, a trust referred to a legal tool used to coordinate cooperative actions among parties that would otherwise compete. Under a trust, member firms placed stock under the control of appointed trustees, who acted collectively to manage operations and limit competition among participants.

[8] Despite extensive scholarly attention, no consensus exists on what—or who—actually drove the antitrust movement. Some scholars have expressed doubt that farmers' cries for help were the result of genuine anticompetitive abuses and have alleged that the real driving force behind antitrust legislation was interest-group lobbying by agricultural groups (Boudreaux and DiLorenzo 1993; Boudreaux, DiLorenzo, and Parker 1995; Dilorenzo 1985; Dilorenzo and High 1988; Gilligan, Marshall, and Weingast 1989). Other scholars, however, reject the idea that agricultural groups were the primary political force behind early antitrust legislation, instead arguing that urban small business interests, especially those facing displacement by more efficient firms, played a more decisive and sustained role (Baxter 1980; John 2017; Stigler 1985). A third group emphasizes that political pressure to respond to perceived monopoly abuses, especially in the wake of widespread public outrage over trusts like Standard Oil and the Whiskey Trust, played a key role in prompting both state and federal antitrust legislation–even in the absence of clear evidence that market concentration had materially increased (Flynn 1988; Hofstadter 1979; Lande 1982; Lande 1988; Letwin 1965; Millon 1988; Werden 2020). A fourth view, associated with Bork (1966) and defended more recently by Crane (2014), holds that the Sherman Act was a "consumer welfare prescription" intended to prevent only those restraints that harmed allocative efficiency, regardless of the political coalitions that gave rise to its passage. A fifth view emphasizes that the Sherman Act was a political compromise among factions with conflicting goals and that its vague language was

reflects the agricultural roots of the movement. As Figure 3 shows, the first states to adopt antitrust laws were concentrated in the largely agricultural Midwestern United States. Monopolization among railroads was an acute concern for farmers, who relied on railroads and railroad-owned grain elevators for the transportation and storage of their crops Thorelli 1955, p. 143; Phillips-Sawyer 2019, p. 4). In particular, farmers alleged that monopolistic railroads charged unfair prices for their services.[9] Libecap (1992) also highlights the role of the Chicago meat packers in depressing cattle prices, which led Midwestern farmers to lobby for antitrust legislation as a way to counteract what they saw as monopsonistic abuses in livestock markets.

States were the first to respond to popular cries for antitrust legislation, with 13 enacting statutes before the federal government.[10] Although state legislatures do not consistently record vote counts,[11] the historical record suggests that state antitrust laws often enjoyed broad, bipartisan support. For example, Texas's 1889 antitrust statute passed unanimously.[12] When that statute was later overturned, the legislature responded by passing a new antitrust law in 1903, which again received overwhelming support—unanimous approval in the Senate and a 103–2 vote in the House.[13]

This state-level action laid the groundwork for antitrust reform at the federal level, beginning with the Sherman Antitrust Act of 1890. There is some evidence that trusts welcomed the passage of the Sherman Act because they believed its passage would thwart state antitrust policy, which they perceived to be a greater threat. For example, Troesken (2000) finds that trust stocks fell significantly in response to state antitrust enforcement, while trust stocks increased in value after two different federal antitrust laws were proposed, suggesting investors favored at least some forms of federal antitrust legislation. The apparent preference of trusts for federal antitrust legislation over state enforcement underscores the initial ineffectiveness of the Sherman Act. In its early years, federal enforcement was notoriously weak, hampered by a lack of resources and political will. It was not until 1903 that Congress appropriated funds to support federal antitrust enforcement (Thorelli 1955, p. 3). Even then, staffing remained minimal; just two professional staff members responsible

designed to paper over disagreement, making any unified theory of original intent—such as Bork's consumer welfare thesis—fundamentally untenable (Grandy 1993; Hazlett 1992).

[9]Economic historians have provided empirical support for farmers' allegations of high railroad freight rates. Higgs (1970) argues that, despite nominal declines in freight rates, the real cost of railroad transportation relative to crop prices remained essentially flat from the 1860s through the 1890s. Responding to Higgs's paper, Aldrich (1980) uses transaction data from the Chicago Board of Trade and published freight rates to argue that real freight rates fell until the early 1880s, then stabilized or rose during the late 1880s and 1890s—a pattern consistent with the political science theory of "relative deprivation," which posits that protest arises not from steady hardship but from the reversal of improving conditions.

[10]Antitrust legislation was neither the first nor the sole form of government regulation that state legislatures pursued in response to political pressure by aggrieved farmers in the mid-nineteenth century. In the 1870s, over a decade before Iowa became the first state to adopt an antitrust statute in 1888, several state legislatures enacted "Granger" laws to regulate the prices and practices of railroads and grain elevators (Noll and Kanazawa 1994). In 1889, state legislatures in several Midwestern states also passed or considered livestock inspection laws to respond to farmer concerns about falling cattle prices and the market power of Chicago meat packers (Libecap 1992).

[11]Texas is an exception; its published session laws include vote counts for each legislative chamber.

[12]Act of March 30, 1889, Chapter 117, 1889 Texas Acts 141.

[13]Act of March 31, 1903, Chapter 94, 1903 Texas Acts 119.

for antitrust enforcement are identified in the 1904 *Department of Justice Register* (Werden 2018, p. 425). Moreover, the Antitrust Division within the U.S. Department of Justice was not created until 1919 (Werden 2018). Thus, despite its landmark status, the Sherman Act's immediate impact on trust-busting was limited by inadequate implementation.

Relative to the federal government's meager enforcement efforts, state antitrust enforcement was robust in the late nineteenth and early twentieth centuries. State enforcement actions were numerous and sometimes targeted major interstate combinations (May 1987, 1990). For example, Standard Oil faced 24 separate state actions (Bringhurst 1979, p. 204).[14] Weak federal enforcement also meant that state action was often the only remedy available to address anticompetive behavior. For example, in *United States v. E. C. Knight Co.*, the U.S. Supreme Court ruled that the Sherman Act did not apply to manufacturing.[15] Through this ruling, the Court effectively made state courts the primary venue for addressing anticompetitive conduct in this crucial sector of the economy. In general, antitrust enforcement lied largely in state hands until the expansion of federal commerce power in the New Deal era (Columbia Law Review 1961).

Federal antitrust law was intended to supplement existing state laws. Senator John Sherman advocated for the passage of the Sherman Act, which was named after him, by arguing that the law would apply at the national level law that already existed within several states. As Hovenkamp (1983, p. 375) writes, "the legislative history of the federal antitrust law indicates that Congress intended to leave state antitrust enforcement more or less intact but to provide an additional federal forum for dealing with restraints of trade which exceeded the jurisdiction of the courts of any particular state." The enduring importance of state antitrust laws, even after the enactment of federal legislation, is reflected in early court decisions that often interpreted state antitrust statutes expansively (Lamb 2001; May 1990).[16] As Brinkerhoff (2017) shows, courts typically focused on whether restraints themselves crossed state lines—not whether their effects did—allowing for meaningful state enforcement even when alleged anticompetitive conduct affected interstate commerce.

[14]May (1987, p. 537) notes that Texas collected $1,623,500 in penalties in a single case against the Texas affiliate of the Standard Oil Company in *Waters-Pierce Oil Co. v. Texas*. The state fined the defendant $1,500 per day for a total of 1,033 days in violation of a 1899 law and $50 per day for a total of 1,480 days in violation of a 1903 law. The fine amounts to about $57 million in 2024 dollars (Federal Reserve Bank of Minneapolis n.d.). The defendant paid 32 percent of the fine and Standard Oil of New Jersey paid the remaining balance. 212 U.S. 86 (1909). Act of May 25, 1899, Chapter 146, 1899 Texas Acts 246. Act of March 31, 1903, Chapter 94, 1903 Texas Acts 119.

[15]156 U.S. 1 (1895).

[16]For example, in *Hammond Packing Co. v. Arkansas*, the U.S. Supreme Court upheld Arkansas's power to condition an Illinois corporation's right to do business in Arkansas on not belonging to a price-fixing conspiracy *anywhere* (the conspiracy was alleged to have occurred in Illinois). 212 U.S. 322 (1909). Likewise, in *International Harvester Co. v. Missouri*, the U.S. Supreme Court upheld Missouri's antitrust action against an out-of-state trust that had formed a monopoly affecting local markets, concluding that the corporation's presence and conduct within the state subjected it to Missouri's jurisdiction. 234 U.S. 199 (1914).

2.2 Existing Literature and Contributions

The effects of antitrust policy on competition have been extensively studied, yet the literature remains divided on the upshot.[17] Several papers studying antitrust policy in historical contexts suggest that the Sherman Act and its initial implementation had limited or even counterproductive effects. These papers argue that landmark antitrust interventions—including early enforcement of the Sherman Act (Stigler 1966); the 1911 dissolutions of Standard Oil, American Tobacco, and American Snuff (Burns 1977); Supreme Court decisions against railroad combinations (Binder 1988); and the 1948 Paramount decision mandating vertical divestiture in the film industry (Gil 2015)—failed to meaningfully alter market structures, suppress anticompetitive behavior, or lower prices. Bittlingmayer (1992, 1993) echoes this skepticism by arguing that uncertainty surrounding antitrust enforcement disrupted financial markets and real investment, potentially causing unintended contractionary effects on the economy. In contrast, subsequent studies argue that historical episodes of antitrust enforcement constrained monopoly power and altered firm behavior. For example, Mullin, Mullin, and Mullin (1995) find that the initiation of the 1911 antitrust suit against U.S. Steel led to stock gains among the conglomerate's customers, indicating expectations that a dissolution would lower steel prices and increase output, and Mullin and Snyder (2021) find that competition increased following the 1952 DuPont patent-licensing remedy and the 1911 breakup of American Tobacco.[18] Moreover, both Prager (1992) and Baker, Frydman, and Hilt (2023) show that financial markets responded strongly to signals of heightened antitrust enforcement, suggesting that investors viewed stricter antitrust policy as a credible threat to firm profitability. Together, these studies highlight ongoing disagreement about whether early antitrust enforcement meaningfully affected markets.

This pattern of disagreement persists in the literature studying antitrust policy in relatively more modern settings.[19] Several studies question the effectiveness of antitrust enforcement, finding

[17] Though I focus on studies of U.S. antitrust policy in this literature review, studies from international contexts also provide insights. Across multiple papers, George Symeonidis finds that cartelization generally harmed economic performance in the United Kingdom, while cartel policy reforms led to more intense competition, which in turn increased labor productivity growth, had no adverse effect on wages, shifted firms toward price rather than non-price competition, encouraged innovation, and sometimes even forced unprofitable cartels to disband (Symeonidis 2000, 2008, 2019, 2024). Geloso (2020) and Crane (2023) also study historical decartelization policies in Canada and Germany, respectively, while Reed et al. (2022) examine modern Mexico and Aguzzoni, Langus, and Motta (2013) study the European Union. Several studies examine how antitrust laws influence economic outcomes across countries, analogous to this paper's focus on variation across U.S. states. They find that antitrust policies reduce firms' complaints about anticompetitive obstacles (Mayo and Schiffer 2006); increase productivity growth (Buccirossi et al. 2013); induce firms to raise investment and external financing in response to greater competition (Dasgupta and Žaldokas 2019); reduce profit margins but encourage mergers as a strategic response to enforcement (Dong, Massa, and Žaldokas 2019); and are effective at constraining market power in nontradable sectors, where competitive pressures from international trade are absent (Besley, Fontana, and Limodio 2021). Another set of papers shows that effective enforcement—not just the adoption of antitrust laws—ultimately shapes outcomes (Bradford, Chilton, and Linos 2025; Hylton and Deng 2007).

[18] The work of Mullin and Snyder (2021) contrasts with the earlier assessment by Burns (1977) that the dissolution of American Tobacco had little effect on the industry's structure or anticompetitive conduct. Mullin and Snyder (2021) do not cite Burns (1977) or discuss why their conclusions differ.

[19] For literature reviews representing opposing perspectives on the effectiveness of antitrust enforcement, see Cran-

that prices often rise following major interventions (Sproul 1993); that enforcement functions as a negative productivity shock (Young and Shughart 2010); and that lax merger policy was not to blame for rising industrial concentration in the late twentieth century (Peltzman 2014). However, other scholars have challenged these conclusions. Bosch and Eckard (1991) show that price-fixing indictments led to substantial equity losses, consistent with the expectation that enforcement would curb monopoly profits, and Shepherd (1982) attributes declining industrial concentration in the mid-twentieth century partly to efficacious antitrust policy. Similarly, Babina et al. (2023) find that federal antitrust cases between 1971 and 2018 increased employment, wages, and business formation, suggesting broad economic benefits. Thus, despite extensive study, no consensus exists on the effectiveness of antitrust enforcement in promoting competition.

Beyond effects on competition, the effect of antitrust policy on innovation has also been a subject of scholarly inquiry, with studies yielding mixed results. A series of studies find that antitrust actions against Bell Labs (Watzinger et al. 2020), the Bell System (Watzinger and Schnitzer 2022), and Microsoft (Thatchenkery and Katila 2023) led to increased innovation—as proxied by patenting behavior—in affected sectors. Likewise, Kwon and Marco (2021) find that antitrust regulation of patent transfers curbs the negative impact of patent consolidation on rivals' innovation. However, other studies complicate this picture. Kang (2025) finds that collusion, which antitrust aims to prevent, raises both the quantity and quality of patents among cartel members. Similarly, Hashmi (2013) reports a mildly negative association between competition and innovation. This unresolved debate highlights the need for further empirical work on when and how antitrust policy affects economic outcomes.

Responding to this need, this paper advances both the literature on historical antitrust enforcement and the broader literature on how antitrust policy affects competition and innovation in several ways. First, unlike previous studies focusing on incremental changes or specific antitrust cases, I examine the overall impact of implementing an antitrust regime where none existed before. This approach allows for a more comprehensive assessment of effects, including potential deterrence, by analyzing broader market outcomes rather than focusing solely on parties involved in particular cases.[20] Second, while many existing studies concentrate on individual industries or a small number of industries, I provide a wider perspective by estimating effects across the entire manufacturing sector. Third, I analyze long-term effects over nearly a century, in contrast to the narrower time windows examined in most of the existing literature. This extended time frame provides a better understanding of the enduring impact of antitrust policy. Fourth, while most existing papers focus on the federal antitrust regime; I instead examine the role of state antitrust policy, allowing me to

dall and Winston (2003) for a skeptical view and Baker (2003) for a pro-enforcement one.

[20] Antitrust laws influence firm behavior by deterring anticompetitive conduct, even among firms never prosecuted for violations. Just as most taxpayers never face an audit but still comply with tax laws because of the threat of scrutiny, antitrust laws may deter firms from engaging in anticompetitive conduct even if enforcement actions are uncommon. While the direct effects of antitrust enforcement are visible in prosecuted cases, deterrence is harder to observe, which underscores the significance of this paper's ability to estimate these broader, indirect effects.

bring new evidence to bear on this ongoing and important economic debate.

This paper also contributes to the broader literature on collusion and firm behavior in the historical United States. A central theme in this literature is that collusion can be both unstable and highly adaptive to institutional and market conditions. Porter (1983) highlights the fragility of cartel arrangements, showing that the nineteenth-century railroad cartel known as the Joint Executive Committee repeatedly broke down in response to unexpected shocks. In other cases, collusion has proven more durable. Alexander (1994), Chicu, Vickers, and Ziebarth (2013), and Vickers and Ziebarth (2014) show that the National Industrial Recovery Act enabled widespread and sustained collusive behavior across multiple industries, with firms reducing price responsiveness to cost changes and ceasing competitive price adjustments altogether. Another strand of the literature examines how collusion interacts with investment and entry. Gross (2020) shows that coordination among Southern railroads facilitated rapid technological standardization, but cartel discipline prevented efficiency gains from reaching consumers. Similarly, Syverson (2024) finds that railroads used strategic duplication to deter entry and temporarily raise prices for grain sellers, but this came at the cost of wasteful overinvestment. Finally, Huang (2025) shows that the Beef Trust manipulated price signals by temporarily raising cattle prices to attract supply, then lowering them, causing more harm to sellers than standard monopsony would predict. Together, these studies underscore that collusive behavior is not only shaped by its legal environment, but can also distort investment, entry, and the information available to market participants.

Finally, this paper builds on work by economic and legal historians, who have written about how states shaped the competitive landscape during the late nineteenth and early twentieth centuries. Most papers in this literature show that state enforcement actions, including lawsuits by attorneys general and shareholders, effectively curtailed anticompetitive conduct during this period (Clay and Troesken 2002; Lamoreaux 2023; Lamoreaux and Phillips-Sawyer 2021; May 1987; Nolette 2012; Phillips-Sawyer 2018; Pratt 1980).[21] I build upon and extend this literature in several important ways. First, I bring quasi-experimental evidence to bear on the effects of state antitrust laws, focusing on the identification of causal effects. Furthermore, unlike much of the existing scholarship that concentrates on legal outcomes such as individual enforcement actions and cases won against trusts in court, I measure "success" by analyzing the extent to which antitrust laws had a discernible effect on important economic indicators. Finally, by comprehensively tracing the evolution of state antitrust laws over time, I offer a more detailed description of state antitrust law in its emergent stage than previously available in the literature. These contributions allow me to present new evidence on how state antitrust laws shaped economic outcomes during the formative era of U.S. competition policy.

[21] Two papers by Werner Troesken offer a perspective that differs from this prevailing view. In a study of the 1889 dissolution of the Chicago Gas Trust, Troesken (1995) shows that the breakup, carried out through a *quo warranto* proceeding under corporate law rather than antitrust law, had little effect on market structure or performance. Similarly, Troesken (1998) argues that competition—not state antitrust enforcement—disciplined the Whiskey Trust.

2.3 Theoretical Predictions

Before turning to the empirical analysis, it is helpful to consider what standard economic theories suggest about the expected effects of state antitrust laws on the outcomes examined in this paper. Because early antitrust policy primarily targeted cartels and other horizontal restraints, state antitrust laws can be understood as interventions designed to shift markets from monopolistic or oligopolistic structures toward more competitive ones. Accordingly, they are expected *ex-ante* to reduce—or at least not increase—firms' profits.[22] If a new state antitrust law meaningfully curtails ongoing anticompetitive behavior or deters new conduct from emerging, markets may shift away from monopoly or oligopoly—where profits are positive—towards perfect competition—where economic profits are zero under standard assumptions. From a welfare perspective, while profits may decline, surplus may be reallocated from producers to consumers, and aggregate surplus may increase. *Ceteris paribus*, increased competition should reduce firms' profits. However, if antitrust laws also enhance efficiency or spur innovation, profits could rise even as market power declines. Further, if enforcement is weak or firms perceive little risk of detection and punishment—or if little anticompetitive conduct existed in the first place—profits may remain unchanged.

Greater competition may also lead to more firms operating in a market. A sizable literature in industrial organization documents a positive relationship between the number of firms and competition (Bresnahan and Reiss 1991; Melitz and Ottaviano 2008; Syverson 2004; Tirole 1988). Standard Cournot models of oligopolistic competition, which show that price converges to marginal cost as the number of firms tends towards infinity, reinforce this view. Additionally, antitrust jurisprudence tends to view a larger number of firms in a market as a sign of increased competition (Hovenkamp and Areeda 2022). Thus, to the extent that state antitrust laws increased competition by weakening the market power of large firms, one would expect them to also increase the number of firms by encouraging new entrants. One important caveat to this theory is that most state antitrust laws explicitly prohibited horizontal price-fixing arrangements but did not affect firms' ability to merge with each other. Thus, if firms responded to a new antitrust law in their state by merging with competitors to avoid prosecution for cartel behavior, state antitrust laws may have reduced the number of firms.[23] My ability to test this theory is partially limited by the fact that the census of manufactures reports establishment counts rather than firm counts. While a firm refers to a distinct business entity, an establishment refers to a single physical location where manufacturing takes place; a single firm can operate multiple establishments. Thus, any observed increase in the number of establishments could stem from new firm entry or growth by existing firms, a distinction that cannot be directly observed in the data.

[22] Similarly, they are expected *ex-ante* to reduce or leave unchanged the retail prices consumers pay. Though not the focus of this paper, I examine effects on food retail prices in Appendix C.

[23] Federal lawmakers similarly declined to limit firms' ability to consolidate with competitors when they passed the Sherman Act in 1890. Lamoreaux (1985) and Bittlingmayer (1985) provide evidence that the wave of mergers that occurred around the turn of the twentieth century was partially spurred by the Sherman Act.

Beyond market structure, economic theory also predicts that increased competition from antitrust policy should affect production and labor demand. Specifically, antitrust laws should lead to higher employment by lifting artificial constraints on output imposed by monopolistic or collusive firms. This prediction reflects a well-established result in industrial organization, which holds that collusion and anticompetitive mergers suppress output (Asker and Nocke 2021; Whinston 2006).[24] In markets where firms had previously restricted output to maintain high prices, antitrust can push firms to expand production. As output rises, so too does the demand for inputs, including labor, resulting in higher employment.

Standard models leave the net effect on wages indeterminate but suggest that labor's share of income should increase if antitrust policy increases competition. For wages, an expansion of output induced by stronger competition should increase labor demand, as discussed above, placing upward pressure on wages. However, heightened competition also reduces firms' profit margins, which may constrain their ability or willingness to raise wages. For the labor share, the total wage bill should grow as output expands and employment rises.[25] At the same time, thinner profit margins imply that the share of total income accruing to capital falls. Taken together, these forces suggest that labor's share of income is likely to increase, even if average wages do not rise appreciably.

A further dimension of firm behavior potentially affected by antitrust policy is innovation. The adoption of an antitrust statute may increase or decrease a firm's incentive to innovate. On one hand, when firms can no longer rely on monopoly power or collusive arrangements to sustain profits, they face stronger pressure to improve their products and processes to stay ahead of rivals. Antitrust policy can also reduce barriers to entry, enabling new firms to challenge incumbents and spurring industry-wide innovation. In such an environment, innovation becomes a key strategy for differentiation and survival.[26] On the other hand, monopolies may foster innovation by investing their substantial resources in research and development (Hashmi 2013; Schumpeter 1942). A possible reconciliation of these two theories posits that the relationship between product market competition and innovation follows an inverted-U shape—that is, innovation increases with competition at low levels of competition, but decreases with competition at high levels (Aghion et al. 2005). According to this view, innovation rises with competition at low levels because closely matched firms are motivated to innovate to gain market advantage, but it falls at high levels of competition because profit margins become too thin to justify the cost of innovation. Overall, these predictions show that antitrust laws' implications for several of the outcomes I study in this paper are theoretically uncertain, underscoring the need for empirical analysis.

[24] Similarly, Jacquemin and Slade (1989) present a model implying collusin reduces employment and wages.

[25] Contrary to this view, Berger, Herkenhoff, and Mongey (2022) argue concentration can raise the labor share.

[26] A twenty-first-century observer might expect the adoption of a state antitrust law to make firms more cautious in their patenting behavior, encouraging a focus on genuine innovation rather than the use of patents for market control. This expectation reflects a modern legal landscape in which tensions between patent and antitrust law are well recognized, particularly when patents are used to exclude rivals or entrench market power. However, early twentieth-century courts generally treated patents as government-sanctioned monopolies that were immune from antitrust scrutiny (Jacobson 2018), making a reduction in patenting motivated by legal risk unlikely at this time.

3 Data Sources and Descriptive Statistics

State Antitrust Laws

I created a historical compendium of state antitrust statutes to trace the development of state antitrust law from 1888 (when the first state antitrust law was enacted) to 1940. My database includes statutes from every state except Alaska and Hawaii, which were not yet states during the study period, allowing for a comprehensive view of subnational competition law across the continental United States. To create this database, I reviewed state session laws and legal codes for each year from 1860 through 1940 and identified statutes outlawing anticompetitive conduct.[27] In total, I identified 168 statutes enacted between 1888 and 1940. This set of statutes includes amendments, which allows me to characterize the evolution of state antitrust regimes over the study period. To my knowledge, no other database documents both the initial enactment and subsequent evolution of state antitrust laws over the late nineteenth and early twentieth centuries. I do not regard statutes applying to a single industry, rather than to commerce more generally, as antitrust statutes in this paper.[28] I find that Iowa became the first state to adopt an antitrust statute in 1888.[29] I also compared my database to contemporaneous compilations of state antitrust laws to ensure accuracy.[30] After identifying state antitrust statutes in effect between 1860 and 1940, I analyzed the content of states' laws. Specifically, I coded the anticompetitive acts outlawed; whether the law made violations a crime, a tort, or both; the minimum and maximum fines and prison sentences authorized; whether the law permitted both a fine and imprisonment to be imposed;

[27] I considered any state statute outlawing restraints against trade, monopolization, horizontal price fixing, horizontal output restrictions, trustee control of corporations, anticompetitve stock purchases, refusals to deal, or some combination thereof to be a state antitrust statute. See Table B1 for definitions of these acts. I began my review with materials from 1860 onward to verify that no relevant statutes had been enacted prior to 1888.

[28] I exclude industry-specific antitrust statutes from my analysis because these laws were limited in scope, and the focus of this paper is to study the broad economic impacts of state antitrust laws. Moreover, applying a uniform identification strategy to statutes targeting very different industries would likely be challenging. For example, an 1897 Florida statute applied antitrust principles to the sale of beef or "other edible animal[s]," while several states, such as Georgia in 1891, passed laws restricting anticompetitive behavior in the business of insurance only. I do not include these statutes in my database of state antitrust laws. Act of June 11, 1897, Chapter 4534, 1897 Florida Acts 60. Act of October 21, 1891, Chapter 745, 1891 Georgia Acts 206.

[29] Scholars have reached different conclusions about which state laws should be considered antitrust statutes. For example, Millon (1990) considers an 1889 Kansas statute to be the United States' first antitrust statute. I consider the 1889 Kansas law to be Kansas' first antitrust statute but recognize Iowa's 1888 law directed at "the punishment of pools, trusts and conspiracies" as being first in the nation. Other scholars also conclude that Iowa was the first state to adopt an antitrust statute (Collins 2013; Dameron 2016). Any time I encountered an accounting of state antitrust statutes that disagreed with my own, I reviewed the differences to confirm the accuracy of my analysis. Act of March 9, 1889, Chapter 257, 1889 Kansas Acts 389. Act of April 26, 1888, Chapter 84, 1888 Iowa Acts 124.

[30] Sources I used to check my work include Forrest (1896), Halle (1899), Bureau of Corporations (1915), and Works Progress Administration (1940). I also compared my compilation of state antitrust statutes to internal memoranda from the now-defunct Bureau of Corporations. The Bureau of Corporations was a government agency that existed from 1903 to 1915, had broad authority to conduct investigations and gather data on the operations of interstate corporations, and predated the Federal Trade Commission. Given the Bureau's focus on investigating large corporations and trusts, I identified a number of internal agency documents relating to state and federal antitrust laws during a visit to the National Archives. These documents helped me ensure the accuracy of my state antitrust database.

any civil damages authorized; the official(s) tasked with the duty to enforce the antitrust law; any exemptions for labor unions and/or the agricultural sector; whether the statute stipulated that every day of violation constituted a separate offense; and any other penalties the statute authorized. Appendix B provides more information on how I identified and coded these laws.

To summarize the features of state antitrust laws with a single, continuous measure of stringency, I constructed an antitrust law index using Principal Component Analysis (PCA). To do this, I coded numeric measures of statutory severity for each state-by-year observation in which an antitrust statute was in force.[31] I then standardized these variables to have mean zero and unit variance to ensure comparability across measures expressed in different units. Next, I used PCA to extract the first principal component, which is the linear combination of the standardized variables that maximizes the proportion of the total variance explained. The resulting first component has positive loadings on all measures of statutory strength, indicating that higher scores reflect more comprehensive prohibitions and stronger penalties.[32] I then rescaled the first principal component to range from 0 to 1, with higher values indicating greater statutory stringency. In the final index, all state-by-year observations without an antitrust law receive an index value of 0, while state-by-year observations with antitrust laws have positive index values ranging from about 0.22 to 1 that reflect their varying levels of statutory severity.

A descriptive analysis of my state antitrust law database reveals that statutes evolved along diverse paths but frequently converged around a few key regulatory priorities. Figure 4 traces the enactment and subsequent trajectories of state antitrust statutes during the study period, illustrating that nearly every state amended its statute at least once, and many modified their statutes repeatedly. Although the content of states' laws varied significantly, some trends emerge. For example, as Figure 5 shows, state antitrust laws most commonly prohibited horizontal arrangements to fix prices or limit quantity. Such provisions were aimed squarely at dismantling cartels, which were the main target of early antitrust law. Several states also included provisions mirroring section 1 of the Sherman Act, which broadly prohibits restraints of trade, in their antitrust statutes. Provisions mirroring section 2 of the Sherman Act, which condemns monopolization, were somewhat less common at the state level, particularly in the first several decades states began adopting antitrust statutes. Several states also prohibited the sale of trust certificates, which were commonly used to coordinate horizontal restraints starting in the late nineteenth century. Fewer states adopted provisions barring firms from buying their competitors' stock or refusing to do business with buyers or sellers that also transacted with a competitor. Moreover, because unions could be construed as combinations that restrained trade through the collective organization of workers, at least 13 states explicitly exempted labor unions from their antitrust statutes to prevent the laws from being used to weaken organized labor. Similarly, at least 11 states explicitly exempted the agricultural sector from their antitrust statutes to protect collective price-setting by farmers.

[31] Table A1 lists and describes the 19 variables I used to construct my antitrust law index.

[32] Table A1 provides the weights associated with the first principal component, which I used to construct my index.

States' antitrust statutes also varied not only in the types of remedies they provided but also in who was empowered to enforce them. As Figure 6 shows, the first states that adopted antitrust laws classified violations as criminal offenses, punishable by fines or imprisonment.[33] In contrast, when antitrust violations were treated as torts, injured parties could seek compensation through private lawsuits. Civil remedies shifted the burden of enforcement away from the state and toward private actors, potentially expanding the reach of antitrust law. Perhaps for this reason, later adopters tended to include means of both civil and criminal recourse in their statutes. Further, as civil enforcement gained traction, states began to replace remedies that enabled compensation for actual harm with more punitive measures aimed at deterrence. As seen in Figure 7, the first crop of states that allowed parties injured by antitrust violations to sue for damages tended to limit damages to the amount of actual losses incurred. Enhanced remedies—such as double or treble damages—became more popular in the early 1900s. States also differed in who they tasked with enforcement. As shown in Figure 8, a small share of states did not designate enforcement to anyone. Most states designated enforcement authority jointly to the Attorney General and District Attorneys, while others relied on only one of these offices. Over time, the share of states assigning joint authority increased, while the share assigning enforcement solely to District Attorneys declined. Variation in enforcement responsibility may have shaped the vigor with which laws were applied, with states assigning responsibility to multiple offices potentially able to mount broader enforcement efforts.

Figure 9 plots my antitrust law index between 1888 and 1940, further underscoring the trend of increasing statutory severity over time. The solid black line traces the mean index value over the study period among states with an antitrust law in force. The steady upward movement of the mean from the late 1880s through the early 1900s reflects the initial wave of enactments and subsequent amendments that expanded prohibitions and strengthened penalties. After about 1910, the mean stabilizes, suggesting that most states had by then settled on a regulatory framework. Moreover, the wide dispersion of index values over time indicates that substantial differences in the comprehensiveness and severity of antitrust laws persisted throughout the study period. These trends are also shown by state in Figure 10.

[33]For states in which antitrust violations were criminal offenses, Figure A2 illustrates the fines and prison sentences authorized under state antitrust statutes between 1888 and 1940. An important caveat is that Figure A2 illustrates the *statutory* minima and maxima for fines and prison sentences. Systematic data on the fines and prison sentences actually imposed under state antitrust laws are not available. However, contemporaneous press coverage suggests that sizable fines were not uncommon, and that at least some individuals were sentenced to jail for antitrust violations. As Figure A2 shows, the median maximum fine was $5,000 among states with antitrust statutes during the study period. (According to historical inflation data, $5,000 in 1890 dollars amounts to approximately $174,981 in 2024 dollars (Federal Reserve Bank of Minneapolis n.d.).) Moreover, as shown in Figure A3, many statutes considered each day a party was in violation of statute to be a separate offense. The median minimum fine hovered around $0 during the study period; that is, the average state did not set a minimum fine for antitrust violations in its antitrust statute. Moreover, as Figure A2 shows, the median maximum prison sentence was 12 months during most years between 1888 and 1840. The year 1897 provides a notable exception; the median maximum prison sentence jumped to 24 months that year when three states (Arkansas, Indiana, and South Carolina) enacted antitrust statutes imposing 10-year maximum prison sentences. As with fines, the median minimum prison sentence also hovered around $0 during the study period, implying that the average state did not set a minimum prison sentence for antitrust violations.

State-by-Industry Manufacturing Outcomes

To assess the impact of state antitrust laws on competition within the manufacturing sector, I draw on state-by-industry tabulations from the census of manufactures, which I digitized in related co-authored work (Barkai, Karger, and Schaller 2025).[34] These data cover the full scope of manufacturing activity in the United States and represent the most comprehensive data source on manufacturing for this period that is disaggregated by both geography and industry. The data span each of the 20 manufacturing censuses that took place between 1850 and 1940, allowing for a systematic examination of long-run trends in manufacturing activity across states and industries.[35] The main outcomes I examine are the number of manufacturing establishments, as well as the profits and labor utilization of these establishments. To obtain profits per state-by-industry cell, I compute

$$\pi_{ist} = Y_{ist} - M_{ist} - W_{ist} \tag{1}$$

where i indexes an industry, s indexes a state, and t indexes the year. Revenue, the value of materials (i.e., inputs), and the total wage bill are denoted by Y_{ict}, M_{ict}, and W_{ict}, respectively. Of course, this approach yields a measure of *accounting* profits rather than *economic* profits. Accounting profits imperfectly proxy real economic returns (Fisher and McGowan 1983). However, the aggregated census of manufactures data I employ in this paper leave me with few feasible alternatives.

Though the census of manufactures provides valuable state-by-industry data, it is not without its limitations. One challenge associated with these data is that the census of manufactures did not include industry codes until 1947, when it adopted the Standard Industrial Classification (SIC) system. Accordingly, I use the industry descriptions included in the census of manufactures tabulations to map industries to their corresponding four-digit SIC codes.[36] To do this, I use the detailed narratives explaining each four-digit SIC code in a contemporaneous version of the *Standard Industrial Classification Manual* (Technical Committee on Industrial Classification 1945). In addition, some censuses report only the number of workers—that is, production laborers such as operatives, "hands," and wage earners—rather than the total number of employees, which would also include salaried staff like managers, superintendents, and clerks. Because comprehensive employee counts do not appear consistently across censuses, I instead rely on the number of workers throughout the analysis.

[34] Establishment-level schedules from the census of manufactures are presumed lost for all years after 1880 and prior to 1929. As Vickers and Ziebarth (2019, p. 1705) describe, "the underlying schedules [from censuses of manufactures taken between 1880 and 1929] have been lost due to accidents such as the fire that destroyed much of the 1890 Population Census, bureaucratic neglect, or active destruction to conserve space at the National Archives." As a result, I use aggregated tabulations that appeared in published census volumes for this research. Without establishment- or firm-level data, some outcomes of interest—such as the Herfindahl-Hirschman Index of industry concentration—are not possible to compute.

[35] Figure A4 shows these years and the number of states included in the state-by-industry data each year.

[36] As Pittman and Werden (1990) show, four-digit SIC codes often diverge from the product market definitions used in antitrust enforcement. However, detailed case-specific market definitions are rarely available across a wide range of industries, making four-digit SIC codes the most practical and consistent alternative.

Trust Status

To identify and characterize major industrial combinations active around the turn of the twentieth century, I rely on *The Truth About the Trusts*, which documents the structure, capitalization, and constituent firms of trusts in the United States at the height of the merger wave (Moody 1904).[37] This source provides detailed information on 445 trusts, which I use to infer each trust's primary industry and assign a four-digit SIC code to enable linkage with other historical economic data. In the absence of comprehensive firm-level data from the study period, I use this source to approximate industry-level concentration, the dimension most likely to determine whether state antitrust laws constituted a binding constraint.[38] *The Truth About the Trusts* has been used in major works such as those by Nutter and Einhorn (1969) and Lamoreaux (1985) and primarily draws on *Moody's Manual of Corporation Securities*, supplemented by the *Commercial & Financial Chronicle*, the *Boston News Bureau*, the *Wall Street Journal*, and other sources (Moody 1904, p. xxii). Subsequent research shows that *The Truth About the Trusts* also relies on the *Manual of Statistics: Stock Exchange Handbook* (Bunting 1971). In keeping with contemporaneous usage of the word "trust," Moody (1904, p. xiii) defines a trust not by its legal form but by its perceived market power; the definition encompasses any combination believed to have the power, intent, or tendency to monopolize, restrict trade, or influence prices.

Figure 11 presents the distribution of capital and establishments by sector for the trusts cataloged in Moody (1904). Panel A, which covers all 445 trusts listed in Moody (1904), highlights the dominance of transportation and utilities in terms of aggregate capitalization. These sectors absorbed a disproportionate share of total investment during the trust era, reflecting both their high fixed costs and the tendency for consolidation to occur where natural monopoly conditions prevailed. Panel B hones in on the 305 entities designated by Moody as "industrial" trusts. Industrial trusts were heavily concentrated in manufacturing, which motivates my decision to focus on that sector

[37] I digitized Table 1 in section VI, *Classified Statistics of All Trusts*, to create a firm-level dataset on major industrial trusts (Moody 1904, pp. 453–477). Trusts are designated as "greater industrial trusts," "lesser industrial trusts," "important industrial trusts in process of reorganiztion or readjustment," "leading franchise trusts" (which are comprised of "telephone and telegraph consolidations" and "electric light, railway, and gas companies"), "the great steam railroad groups," or "'allied independent' steam railroad systems."

[38] I also considered using the surviving schedules from the 1850–1880 censuses of manufactures (Hornbeck et al. 2024) to approximate industry-level concentration in the manufacturing sector prior to the rise of the antitrust movement, but I ultimately chose not to do so, for reasons discussed below. The surviving schedules from 1850 to 1880 provide the last nationwide, establishment-level data on U.S. manufacturing prior to 1929 because establishment-level schedules from the census of manufactures are presumed lost for all years after 1880 and before 1929. However, a key limitation of these data is the absence of firm identifiers, which prevents aggregation of establishments under common ownership. As a result, attempts to compute a Herfindahl-Hirschman Index (HHI) will likely understate true market concentration, especially in cases where many establishments are in fact owned by a single firm. While an industry with a small number of establishments correctly indicates high concentration, a diffuse structure of small establishments need not imply a competitive market if ownership is consolidated. The limitations of establishment-based HHI are reinforced by Figure A5, which shows that trust-affiliated industries had consistently *lower* HHI than non–trust-affiliated industries from 1850 to 1880—the opposite of what would be expected if concentration were measured accurately. This pattern suggests that the absence of firm identifiers is a material concern, motivating my decision not to measure pre-enactment concentration using these data.

throughout the analysis. The magnitude of these trusts' economic footprint is remarkable. In 1904, four-digit SIC industries that included at least one trust accounted for approximately 59 percent of total manufacturing revenue. This fact highlights the outsized role that industrial trusts played in the early twentieth-century economy.

Stock Returns

To understand how the stock market reacted to the passage of state antitrust laws, I use data on daily stock returns to the Dow Jones composite portfolio (Schwert 1990). The dataset covers 1885 to 1962 and reports a daily time series over this period, capturing average returns, capital gains, and dividend yields for the composite portfolio. The index is composed of 12 to 50 industrial and transportation stocks. While the index's composition changed over time, it consistently aimed to include the most prominent stocks in terms of trading volume and market capitalization. The resulting data thus enable analysis of market-level reactions to changes in antitrust policy. Because stock prices reflect investors' expectations about firms' future earnings, stock market responses to the passage of state antitrust statutes can provide insight into whether these laws were anticipated to constrain or enhance profitability.[39] One limitation of using these data to detect changes in investors' expectations is that the index is reported only at the national level and does not identify which constituent firms had significant operations in states enacting antitrust laws. Nevertheless, the index allows for inference about how investors, in the aggregate, perceived the broader economic implications of state antitrust legislation.

Patents

To identify the effects of state antitrust laws on innovation, I draw on data from the Comprehensive Universe of U.S. Patents (CUSP) (Berkes 2018). The CUSP database contains the full population of patents issued by the U.S. Patent and Trademark Office (USPTO) between 1836 and 2016, enabling consistent measurement of inventive activity over nearly two centuries. This long time horizon makes it possible to observe both the pre- and post-enactment periods of state antitrust laws and to analyze the evolution of innovation at geographically disaggregated levels. The dataset provides a rich set of variables for each patent, including issue and filing years, technology classifications, backward and forward citations, and detailed information on inventors and assignees such as names and geocoded addresses. Berkes (2018) constructed the database by merging records from multiple sources, including digitized USPTO files, state-level archives, and optical character recognition of historical patent documents. The resulting dataset has been extensively validated through comparisons with official USPTO statistics and other widely used historical sources, such as HistPat (Petralia, Balland, and Rigby 2016).

[39] As discussed in Section 2.2, a number of previous studies have documented negative stock market reactions to the implementation of antitrust policy(e.g., Baker, Frydman, and Hilt 2023; Prager 1992).

Newspaper Articles on State Antitrust Topics

I assembled a collection of 118,560 historical newspaper articles that mention one or more state antitrust laws during the study period.[40] To identify relevant articles, I implemented a search strategy designed to cast a wide net while still targeting content explicitly related to state antitrust laws. Specifically, I searched for articles that (1) contain either `antitrust` or `anti-trust`, (2) contain a state name or the title of a named state antitrust law (e.g., `cartwright` in reference to California's Cartwright Act, `donnelly` in reference to New York's Donnelly Act, or `valentine` in reference to Ohio's Valentine Act), and (3) were published between 1850 and 1940. The resulting corpus spans a wide range of geographies, capturing both local reporting in small-town papers and coverage in major metropolitan dailies.[41] It also complements the statutory and economic data I use by revealing how legal changes were communicated to and interpreted by the public.

Figure 12 plots the number of articles in my dataset by year of publication and by the treatment status of the state mentioned in the article.[42] The figure corroborates that the collection of newspaper articles I assembled generally reflects discussion of state antitrust laws. Because never-treated states never passed a statute, journalists in those states had fewer reasons to cover antitrust-related topics than their counterparts in states that eventually adopted such laws.[43] Accordingly, these states have very few mentions in the collection of newspaper articles I assembled, particularly relative to ever-treated states. These articles allow me to validate that the passage of a statute was publicly visible—that is, people in treated states were likely aware of the law's existence. Beyond documenting visibility, these articles also provide a window into contemporary enforcement activity, political debate, and public attitudes toward state antitrust laws.

[40] These articles come from two digital newspaper archives. The first is the *Chronicling America* project established by the Library of Congress, and the second is the ProQuest Historical Newspapers database. Together, these sources' rich holdings comprise over 73 million digitized pages. *Chronicling America* includes about 21 million pages from 4,051 newspapers, many of which are small-town publications (Library of Congress 2024). ProQuest, meanwhile, includes about 52 million pages from 39 major newspapers (ProQuest 2024). Just three newspapers appear in both datasets; I remove duplicate articles from these newspapers to ensure no publication is over-represented. Combining articles from these two databases provides comprehensive coverage of newspaper media during the study period. It is also worth noting that *Chronicling America* provides Optical Character Recognition (OCR) text at the page level rather than for individual articles, while ProQuest provides article-level data but only makes OCR text available for an "abstract" consisting of roughly the first paragraph of each article. Importantly, these differences in OCR structure do not affect my ability to produce accurate article counts across time and space.

[41] Geographic coverage is not perfectly even across states and years, reflecting the holdings and digitization priorities of the two archives. *Chronicling America* has more extensive coverage in some states, while ProQuest emphasizes large urban centers. Moreover, as with all keyword-based searches, some relevant articles may be missed if they use unexpected terminology, and some irrelevant articles may be included if keywords appear in unrelated contexts.

[42] Figure A6 provides several examples of articles that appear in my dataset. As the examples illustrate, newspaper coverage of state antitrust laws ranged from brief notices announcing newly passed statutes, to opinion pieces lamenting weak enforcement, to detailed accounts of court cases, trials, and other enforcement actions.

[43] Articles mentioning antitrust law in never-treated states may include, for example, editorials calling on the state's legislature to pass an antitrust statute. Some false positives are likely also included in the dataset and may reflect mentions of *federal* antitrust enforcement within never-treated states (and ever-treated states). To address this issue, I attempt to identify and remove articles containing words and names (such as the names of U.S. attorneys general) that likely reflected federal actions.

Controls

Throughout the analysis, I control for the population, literacy rate, median occupational score, and personal income in state s and year t to account for potential confounding demographic and economic conditions. I constructed the first three variables from full-count U.S. population census microdata from 1850 to 1940 (Ruggles et al. 2021), and I created a composite series on state personal income drawn from four sources (Bureau of Economic Analysis 2025; Easterlin 1960; Easterlin 1957; Klein 2009).[44] Because the control variables are observed in years that do not always align with the census of manufactures, I linearly interpolate values between data points to generate consistent annual controls. Population captures local market size, literacy proxies human capital, and occupational score—a widely used metric in historical research—serves as a proxy for socioeconomic status in the absence of individual income data. However, occupational score may reflect social prestige or long-run status more than current economic conditions. Thus, to more directly account for contemporaneous state-level economic performance, I also control for personal income.

In robustness checks, I control for a number of additional variables to rule out alternative explanations for the results I document. The first of these is the share of employment in agriculture, which I compute from full-count U.S. population census microdata (Ruggles et al. 2021). Given the agrarian roots of the antitrust movement, states with larger agricultural sectors likely faced stronger pressure to enact and enforce antitrust laws; controlling for agricultural employment share helps account for these underlying economic and political dynamics. Second, information I collected on whether the state's constitution included an anti-monopoly provision helps to fully account for the legal environment vis-à-vis competition policy during the study period.[45] These provisions were short (63 words on average) and typically quite general in nature. Most frequently, these provisions would simply direct the state legislature to pass laws to regulate monopolies. They would also commonly decry the existence of monopolies or declare combinations, trusts, and/or other restraints of trade to be illegal. These provisions made restraints of trade neither criminal nor tortious, and designating a state officer, such as the Attorney General, to enforce a constitutional provision against monopolies, was rare. Third, using data from Barkai, Karger, and Schaller (2025), I control for antitrust cases initiated by the U.S. Department of Justice during the study period. These data capture contemporaneous federal enforcement activity, which could influence manufacturing outcomes either directly—by targeting manufacturing firms—or indirectly. Including these controls increases confidence that my estimates isolate the effects of state antitrust laws from other factors that could confound the results.

[44] Figure A7 shows how I pieced these sources together over time.

[45] I searched a database of historical state constitutions for keywords related to competition policy to identify all anti-monopoly provisions in state constitutions from 1776 (when the first such provision was ratified) through 1940. Figure A8 illustrates states' adoption of anti-monopoly constitutional provisions. Figure A9 illustrates the contents of states' anti-monopoly constitutional provisions, where each type of provision is defined as in Table A2.

4 Identification Strategy

The following two-way fixed effects model provides a framework for identifying the effects of state antitrust laws on manufacturing outcomes. Specifically, I base my analysis on:

$$y_{ist} = \beta_0 + \beta_1(statelaw_s \times post_{st}) + \Gamma X_{st} + \delta_{is} + \delta_{it} + \varepsilon_{ist} \tag{2}$$

where i denotes an industry, s denotes a state, and t denotes the year. The model includes state-by-industry fixed effects δ_{is}, industry-by-year fixed effects δ_{it}, and a vector of time-varying state-level controls X_{st} in most specifications. The variable y_{ist} denotes the outcome, $statelaw_s$ indicates whether state s adopted an antitrust law, and $post_{st}$ indicates the time period after which a state antitrust law was adopted.[46] Because no antitrust statutes existed in the United States prior to 1888, this approach allows for comparison to a period without any antitrust statutes, which papers studying antitrust in modern settings are unable to do.[47] Throughout the analysis, I cluster standard errors by state to correct for serial correlation and because treatment occurs at the state level. Because the timing of effects is of interest, I also employ event study models in this paper. The two-way fixed effects version of the event study model I estimate is given by:

$$y_{ist} = \sum_{j \neq 0} \beta_j 1[t = j] \times statelaw_s + \Gamma X_{st} + \delta_{is} + \delta_{it} + \varepsilon_{ist} \tag{3}$$

where variables are defined as above.

Recent research in econometrics has documented that two-way fixed effects specifications may produce biased estimates of average treatment effects in difference-in-differences and event study models when treatment timing is staggered and treatment effects may be heterogeneous across groups or over time (Borusyak, Jaravel, and Spiess 2024; Callaway and Sant'Anna 2021; Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021). Given that states adopted antitrust statutes at different times, this concern is relevant in the present context. As a result, I estimate average treatment effects using a stacked difference-in-differences design that accommodates variation in treatment timing across states (Baker, Larcker, and Wang 2022; Cengiz et al. 2019; Deshpande and Li 2019; Gormley and Matsa 2011). This approach mitigates the bias from negative weighting and treatment effect heterogeneity, yielding more credible estimates of the impact of state antitrust laws. Two-way fixed effects estimates produce similar results.

To implement this methodology, I organize my data into stacks based on year-of-enactment

[46] As illustrated by Figure 4, several states' antitrust laws were overturned by a court ruling or repealed by a legislative act during the period I study. Several state legislatures also repealed their states' antitrust laws during the study period. To avoid treating these states in the same manner as states that never adopted any antitrust statute, I exclude states whose antitrust laws were overturned or repealed from my analysis.

[47] Though Iowa became the first state to adopt an antitrust statute in 1888, courts had long declined to enforce contracts in restraint of trade prior by 1888. Antitrust statutes supplemented these common law principles by making restraints against trade crimes (and often torts as well).

cohort. That is, each stack consists of states that adopted their antitrust statute at the same time. For any given stack, both never-treated and not-yet-treated states serve as controls. I then append the stacks together and estimate the following equation:

$$y_{aist} = \beta_0 + \beta_1(statelaw_s \times post_{st}) + \Gamma X_{st} + \delta_{as} + \delta_{at} + \delta_{is} + \delta_{it} + \varepsilon_{aist} \tag{4}$$

where a denotes a stack and other variables are defined as above. Equation 4 includes several sets of fixed effects to account for time-invariant and time-varying unobserved heterogeneity. The stack-by-state fixed effects δ_{as} control for permanent differences across states within a given stack, such as differences in state-level economic structure, political preferences, legal traditions, or enforcement capacity that persist over time. The stack-by-year fixed effects δ_{at} absorb year-specific shocks that are common to all units within a stack, such as macroeconomic conditions. The state-by-industry fixed effects δ_{is} account for persistent differences in industry structure or other industry-specific economic characteristics across states, while the industry-by-year fixed effects δ_{it} control for time-varying shocks that affect specific industries nationally, such as technological change or national demand fluctuations. Together, these fixed effects allow me to isolate treatment effects by leveraging within-stack variation while controlling for a rich set of potential confounders. Throughout the paper, I weight observations by both the inverse frequency of the census of manufactures and revenue. Specifically, observations from decennial censuses receive a weight of 1, those from quinquennial censuses receive a weight of 0.5, and those from biennial censuses receive a weight of 0.2. I then weight observations by their share of manufacturing revenue in the observed census year.[48]

The key identification assumption underlying the difference-in-differences models I employ is that, in the absence of treatment, treated state-by-industry cells would have followed similar trends as control state-by-industry cells within the same industry and year. This assumption is made conditional on state-by-industry and industry-by-year fixed effects. The assumption is most credible if unobserved shocks affecting treated and control units within the same industry evolve similarly over time. While this assumption is fundamentally untestable, I provide support for its plausibility in several ways. First, I present event study estimates to assess pre-trends. Second, I show that observable characteristics evolve similarly across treated and control states prior to treatment.[49] Finally, I test the sensitivity of my estimates to alternative specifications and control variables. Together, these strategies increase confidence in the plausibility of the parallel trends assumption.

My identification strategy has several limitations that merit discussion. First, manufacturing

[48]This period saw rapid economic growth, so weighting by raw or inflation-adjusted revenue disproportionately emphasizes later years. To mitigate this, I weight each observation by its share of total revenue in its census year.

[49]Table A3 compares pre-treatment characteristics in 1880—the last census before the first state antitrust law was adopted—between states that ever adopted an antitrust law and those that never did. Table A4 compares early- and late-adopting states. These comparisons reveal that treated and control states were broadly similar across a range of demographic, economic, and political characteristics prior to any state adopting an antitrust statute. Although a few differences are statistically significant, most variables show no significant differences. This balance provides suggestive evidence that treated and control states would have followed similar trends in the absence of treatment.

data are available only intermittently (every two, five, or ten years), which may obscure treatment effects that unfold rapidly following a law's adoption. Second, treatment effects may not be contained within state borders. If firms respond to antitrust laws by relocating or adjusting their behavior in untreated neighboring states, these spillovers could potentially bias estimates towards zero. Third, although my design controls for rich sets of fixed effects and observable covariates, I cannot rule out the possibility of selection into treatment. States that adopted antitrust laws may have differed systematically from those that did not in unobservable ways that also affect manufacturing outcomes. Finally, while this historical setting provides a rare opportunity to study the origins of antitrust policy, the economic principles, case law, and politics of antitrust policy have changed significantly since the late nineteenth century. As a result, my findings may not generalize to modern contexts in which antitrust law is embedded in a more complex regulatory environment.

5 Results

The initial enactment and ongoing enforcement of state antitrust laws generated noticeable coverage in contemporaneous newspapers, providing evidence that these statutes were publicly salient and known to economic actors. Figure 13 presents an event study estimating the effect of state antitrust law enactments on the number of newspaper articles discussing state antitrust laws. In the years leading up to enactment, the estimates remain close to zero until roughly five years prior, when mentions begin to rise. This pre-enactment uptick suggests that public discourse may have anticipated the introduction and passage of an antitrust statute. Following enactment, there is a sharp and sustained increase in antitrust mentions, suggesting that these laws received meaningful public attention. This pattern corroborates the idea that individuals and firms were aware of the new statutes and that their economic behavior could plausibly have been influenced in response.

The widespread visibility and public awareness of state antitrust laws raise the question of whether they produced measurable effects on markets. I find that they did, but that their impact was to entrench rather than erode industrial interests. In particular, I find that these laws induced growth in the manufacturing sector by increasing the number of manufacturing establishments, but, as described in further detail in the next paragraph, these new establishments most likely reflected expanded operations by incumbent firms rather than new entrants. As shown in column (4) of Table 1, the number of establishments rose by approximately 10 percent following the adoption of these laws. Notably, this expansion was concentrated in industries that did not have a trust. In these non-trust industries, the number of establishments increased by roughly 16 percent, while trust-affiliated industries saw no comparable growth. This pattern suggests that state antitrust laws did not curtail trusts' market power enough to allow new entrants to emerge in industries dominated by trusts, but they did foster growth in less concentrated industries. Event study estimates presented in Panel A of Figure 14 reveal that the establishment growth was relatively steady but intensified over time, indicating that the effects of these laws accumulated gradually rather than generating

immediate shifts.

Although it is not fully clear whether the observed growth in manufacturing establishments reflects new firm entry or the expansion of existing firms, available evidence points toward the latter. To explore this, I examine changes in ownership patterns—specifically, the number of proprietors per county-by-industry cell. Unfortunately, this variable is only available in a limited set of census years.[50] If incumbent firms expanded by opening new establishments without taking on additional owners, we would expect a decline in the average number of proprietors per establishment. Consistent with this possibility, the estimated effect of antitrust law adoption on the average number of proprietors per establishment is negative across all specifications in Table 1, though statistically insignificant. However, more pronounced effects appear when the sample is restricted to industries without a trust. As shown in Table 2, the number of proprietors per establishment declined by roughly 17 percent in these industries—a magnitude similar to the 16 percent increase in the number of establishments. This pattern suggests that, in sectors not dominated by trusts, incumbent firms may have responded to antitrust statutes by expanding their operational footprints through internal growth, thereby reducing the proprietors-to-establishment ratio. Taken together, these findings offer suggestive evidence that the expansion in non-trust industries was driven not by *de novo* entry but by the extensive-margin expansion of existing firms. However, this interpretation should be viewed with caution given the limited temporal coverage of the ownership data.

Although the number of establishments increased, I find no significant effects on profits or employment, suggesting that incumbent firms were not forced to materially alter their pricing or production strategies in response to state antitrust laws. These null results hold across the full sample (see Panel B and Panel D in Table 1) as well as when disaggregated by trust status (see Panel B and Panel D in Table 2). Event studies are also consistently flat (see Panel B in Figure 14 and Panel A in in Figure 15). The stability of profits and employment across specifications indicates that real output likely remained unchanged following the adoption of these laws. This, in turn, implies—at least at the aggregate level examined here—that markets did not transition from cartelized to more competitive structures as a result of new antitrust laws. Firms did not hire more workers to meet growing production needs, nor did they experience meaningful declines in profitability.[51] These patterns are difficult to reconcile with a strong competitive shock and instead

[50]The relatively small number of observations in panel C of Table 1 reflects the fact that the census of manufactures did not include questions about ownership in all years. Data on the number of proprietors are available in 1900, 1904, 1909, 1914, 1919, 1921, 1923, and 1939. By 1900, most states that would adopt an antitrust law had already done so, so this analysis is underpowered.

[51]I also find null effects for productivity and trust incorporations. Across a range of TFP estimation methods, I find no evidence that the enactment of state antitrust laws affected productivity in Figure A10. I also show that the enactment of state antitrust laws likely played a minimal role in the Great Merger Movement. The Great Merger Movement was a period of rapid consolidation from about 1895 to 1904, when thousands of firms were combined into large trusts and holding companies (Lamoreaux 1985). As Figure A11 shows, much of this consolidation occurred when firms reincorporated in holding-company-friendly states, such as New Jersey. Figure A12 shows no effect on incorporations by trusts after the enactment of a state antitrust law that is statistically distinguishable from zero. This null effect may reflect trusts' expectation that state antitrust laws posed a limited threat to their profitability.

suggest that firms with market power adapted to the new legal environment without meaningfully changing their behavior. Rather than eroding rents, the laws may have formalized certain boundaries on overtly collusive behavior while allowing existing structures of market power to persist.

Another notable and robust finding is the decline in labor share following the adoption of state antitrust laws.[52] As shown in Panel F of Table 1, the labor share decreased by an average of 4 percent post-enactment. This decline runs counter to the prediction that increased competition would raise labor's share of income. Figure 16 reveals that this decline is driven by increases in revenue outpacing increases in total compensation. Importantly, this pattern is driven by trust-affiliated industries, as demonstrated in Panel F of Table 2 The fact that revenues grew faster than compensation indicates that capital, rather than labor, captured the gains from expansion. This result calls the effectiveness of early state antitrust laws into question and suggests that the net effect of state antitrust laws may have been to entrench rather than disrupt market power. Rather than reducing the market power of dominant firms, the implementation of these laws appears to have left trust-affiliated firms largely unscathed—and may even have reinforced their position. This interpretation is consistent with the broader pattern of muted responses in profits and employment and highlights the limits of state antitrust policy in curbing entrenched economic power.

A reasonable next question to ask is whether differences in enforcement shaped these outcomes. To investigate this, I use contemporaneous newspaper discussions of state antitrust policy to distinguish between high- and low-enforcement states and estimate treatment effects separately for each group relative to untreated states.[53] I rely on newspaper coverage as a proxy for enforcement intensity because records of cases brought by state attorneys general and district attorneys are not consistently available, and data on private enforcement are even scarcer. While imperfect, this approach provides the most consistent and broadly comparable proxy available for gauging state-level enforcement intensity during this period. The results in Table 3 suggest that stronger enforcement, as measured by this proxy, did not systematically generate stronger procompetitive effects. High- and low-enforcement states exhibit only minor differences in how enactment affected the number of establishments, average profits, labor demand, average wages, and the labor share. One notable result is that the average number of proprietors per establishment decreased by about 18 percent

Alternatively, another possible reason for the null effect is that state of incorporation typically did not constrain state antitrust jurisdiction—states could generally bring antitrust suits if a firm conducted business within their borders regardless of where the firm was incorporated.

[52] I compute the labor share as the ratio of total wages to total revenue in industry i, state s, and year t. Results are similar if I instead use value added in the denominator, but I rely on revenue because value added is negative in some cases, complicating interpretation. Results using value added in the denominator are available in Figure A13.

[53] For each year a statute was in effect, I classify whether a state's newspaper mentions per capita were above or below the annual median among treated states. Importantly, I use newspaper mentions *per capita* to avoid conflating enforcement activity with population size, since larger states naturally have more and larger newspapers that produce more articles. Using a per capita measure ensures that the proxy reflects the relative importance of enforcement activity in a way that is comparable across states of very different sizes. Next, I classify a state as high enforcement if the share of its treated years spent above the annual median is greater than or equal to the median share across states.

after enactment in low-enforcement states, pointing to a possible pattern of consolidation in the ownership of manufacturing establishments in an environment where regulatory oversight did relatively little to constrain market power. Overall, however, the evidence indicates that higher levels of newspaper-documented enforcement activity did not translate into a clearer pattern of improved competitive outcomes in manufacturing.

In Table 4, I extend the analysis to a continuous difference-in-differences framework to see if more stringent antitrust statutes produced stronger procompetitive effects. I do so using the Borusyak, Jaravel, and Spiess (2024) estimator, which accommodates staggered treatment timing and continuous treatment.[54] I start by replicating the stacked difference-in-differences estimates using the Borusyak, Jaravel, and Spiess (2024) estimator in column (1). The results are broadly consistent, though the Borusyak, Jaravel, and Spiess (2024) estimator produces estimates with larger magnitudes than the stacked approach for the number of establishments and labor's share of income. Column (2) provides the continuous difference-in-differences specification that allows treatment effects to vary with the stringency of state antitrust laws. In this specification, the baseline ATT represents the projected effect of enactment when the index equals zero. Because an index of zero corresponds to having no antitrust statute at all, this estimate should be interpreted as an extrapolated intercept rather than a realized effect for untreated states. Moreover the "Antitrust Index" coefficient measures the marginal change in the treatment effect for a one-unit increase in the antitrust law index, allowing me to assess whether statutory intensity affects the magnitude of estimated treatment effects.

Overall, the results in Table 4 do not provide consistent evidence that greater statutory stringency translated into stronger procompetitive effects. For two outcomes, the coefficient on statutory stringency is insignificant, while for two others it is statistically significant but carries the opposite sign of what would be expected if stronger antitrust laws enhanced competition. Specifically, the coefficient is insignificant for the number of establishments (Panel A) and average wages (Panel D). In addition, Panel B and Panel E show that more stringent antitrust laws are actually associated with *higher* profits and a *lower* labor share, respectively. Results for average employment (Panel C) tell a somewhat different story. For this outcome, greater statutory stringency is associated with greater employment per manufacturing establishment. However, because the projected constant is negative, this positive association only emerges at relatively high levels of statutory stringency; hence, the result does not reflect a broad-based procompetitive effect. Together, the continuous difference-in-

[54]The Borusyak, Jaravel, and Spiess (2024) estimator uses an "imputation" approach: it first estimates untreated potential outcomes using only not-yet-treated units, imputes counterfactual outcomes for treated units, and then averages the resulting treatment effects. Unlike the stacked difference-in-differences approach, which restructures the panel into cohort-specific event-time datasets, the Borusyak, Jaravel, and Spiess (2024) approach works directly on the full panel and provides unbiased estimates under arbitrary treatment effect heterogeneity. The estimator cannot simultaneously accommodate both state-by-industry and industry-by-year fixed effects because imputation requires sufficient untreated variation within each fixed-effect cell. In this analysis I therefore include state-by-industry fixed effects only, which are more granular than industry-by-year in my setting and capture the key heterogeneity across industries within states.

differences estimates provide little evidence that more stringent statutes systematically generated stronger procompetitive outcomes.

The absence of a discernible stock market reaction to the passage of state antitrust laws further supports the conclusion that the enactment of state antitrust laws did not represent a major competitive shock.[55] As shown in Figure 17, average daily returns to the Dow Jones composite portfolio remained close to zero in the days surrounding passage, with confidence intervals encompassing zero throughout the event window. If these laws had been expected to substantially erode incumbent firms' market power or profitability, one would anticipate negative abnormal returns upon enactment. It is worth noting that this stock market analysis captures short-run investor responses over a matter of days, whereas the manufacturing analysis examines outcomes that unfold over decades; the two therefore reflect very different dimensions of firms' reactions to antitrust legislation. Nonetheless, the lack of such a response suggests that investors did not view the legislation as a credible threat to established market structures, a finding consistent with the muted effects on profits and employment documented above.

The main results are robust to a range of alternative specifications and weighting strategies. As shown in Table 5, the positive effect of antitrust laws on the number of manufacturing establishments and the negative effect on the labor share remain statistically significant across all robustness checks. These include alternative weighting schemes based on the number of establishments (column (1)) and employment (column (2)). In column (3), I also control for the share of agricultural employment given that treated states were more agricultural than untreated states. In column (4), I control for whether the state constitution contained an anti-monopoly provision to account for other features of state competition policy that could have plausibly influenced the results. The consistency of results across these specifications indicates that the findings are not sensitive to modeling choices or to specific features of the data.

Figure 18 reveals heterogeneity in the effects of state antitrust laws across manufacturing industries. Across most sectors, the number of establishments increased significantly, consistent with incumbent firms expanding their operational footprints. By contrast, industries that were historically more concentrated—such as tobacco and petroleum—exhibited little to no change in establishment counts, suggesting that dominant firms in these sectors were less affected by state antitrust laws. At the same time, the labor share declined across a broad range of industries, with particularly pronounced reductions in sectors like food products and tobacco products. These patterns suggest that while state antitrust statutes may have prompted expansion in some parts of the manufacturing

[55]This analysis is descriptive rather than causal and should be interpreted cautiously. If the timing of antitrust law enactment coincided with other events that affected firms' profitability, examining changes in stock prices during the week of enactment may not capture the true expected effect of the laws. Moreover, the Dow Jones composite portfolio reflects large, nationally traded firms, many of which may have had limited exposure to specific state laws. Smaller, regionally focused firms, which were more likely to be directly affected, are not included in the index. Finally, this exercise does not capture reactions that occurred earlier (in anticipation of passage) or later (during enforcement), and averaging across all events may mask heterogeneous effects across states, industries, or firm types.

sector, they largely failed to shift the distribution of industrial output.

Finally, the enactment of state antitrust laws resulted in an increase in both assignee (firm-sponsored) and inventor patents. Specifically, assignee patents increased by about 7 percent and inventor patents increased by about 5 percent in the 40 years following the enactment of an antitrust statute, as shown in Table 6. Event study estimates in Figure 19 show that both types of patents started to increase in the two years prior to enactment, potentially reflecting anticipatory effects in states that would soon be treated.[56] The statistically significant increase in assignee patents was sustained for about half a decade after enactment before tapering off slightly and then continuing to increase slowly in later years. For inventor patents, the increase emerged more gradually, with statistically significant effects not appearing until about two decades after enactment. This divergence in timing and magnitude aligns with theoretical expectations about which type of patenting should be most responsive to changes in antitrust policy. Because assignee patents are generated within firms, they are more directly tied to product market conditions than inventor patents. The stronger and earlier increase in assignee patents supports the view that state antitrust laws reinforced firms' innovative capacity, and that this innovation was concentrated among incumbent firms rather than new entrants. Together with evidence that state antitrust laws entrenched rather than curtailed dominant firms' market power, this pattern accords with the view that "bigness"—rather than intensified competition—fuels innovation, insofar as large firms are able to invest considerable resources in research and development.

6 Conclusion

State antitrust statutes emerged amid growing concerns about monopoly power, but their economic impact reveals a more complex legacy. In this paper, I argue that state antitrust laws did not meaningfully curtail anticompetitive conduct and in fact may have entrenched the market power of dominant firms. Though these statutes increased the number of manufacturing establishments by about 10 percent, this growth appears to have been driven by the expansion of incumbent firms rather than new entry. Moreover, this expansion was accompanied by a decline in the labor share, especially in trust-affiliated industries, suggesting that dominant firms retained their market power and captured the gains from expanded activity. I find no evidence of increased employment or reduced profits, indicating that firms made few, if any, adjustments to their pricing or production strategies following the adoption of state antitrust laws. There is little indication that higher enforcement intensity or more stringent statutes moderated these effects. These findings are consistent with the idea that Progressive Era regulation was often promoted and shaped by large corporations themselves, rather than by reformers acting in the public interest to restrain business power (Kolko 1963). Ultimately, the broader lesson from states' early forays into antitrust law is that legal re-

[56] This timing coincides with the increased newspaper coverage of antitrust policy I observe in states on the verge of enactment, as shown in Figure 13.

forms alone, absent effective enforcement and insulation from regulatory capture, are unlikely to shift markets towards competitive equilibria.

My findings also speak directly to recent critiques arguing that antitrust is "atomistic"—i.e., oriented around assessing the competitive effects of discrete acts in isolation even though market outcomes are the result of multiple acts by multiple parties (Feldman and Lemley 2022). While these critiques focus on modern antitrust policy, the evidence I present suggests that the limitations of atomistic antitrust were evident in earlier periods as well. Though individual state cases may well have succeeded in curbing particular practices, my results suggest that state antitrust laws did not meaningfully restrain anticompetitive conduct in aggregate. Stated differently, the cumulative effect of case-by-case enforcement was insufficient to redirect industries toward competitive equilibria. These findings highlight the enduring challenge of designing antitrust regimes that can move beyond isolated acts to address the deeper structures of market power.

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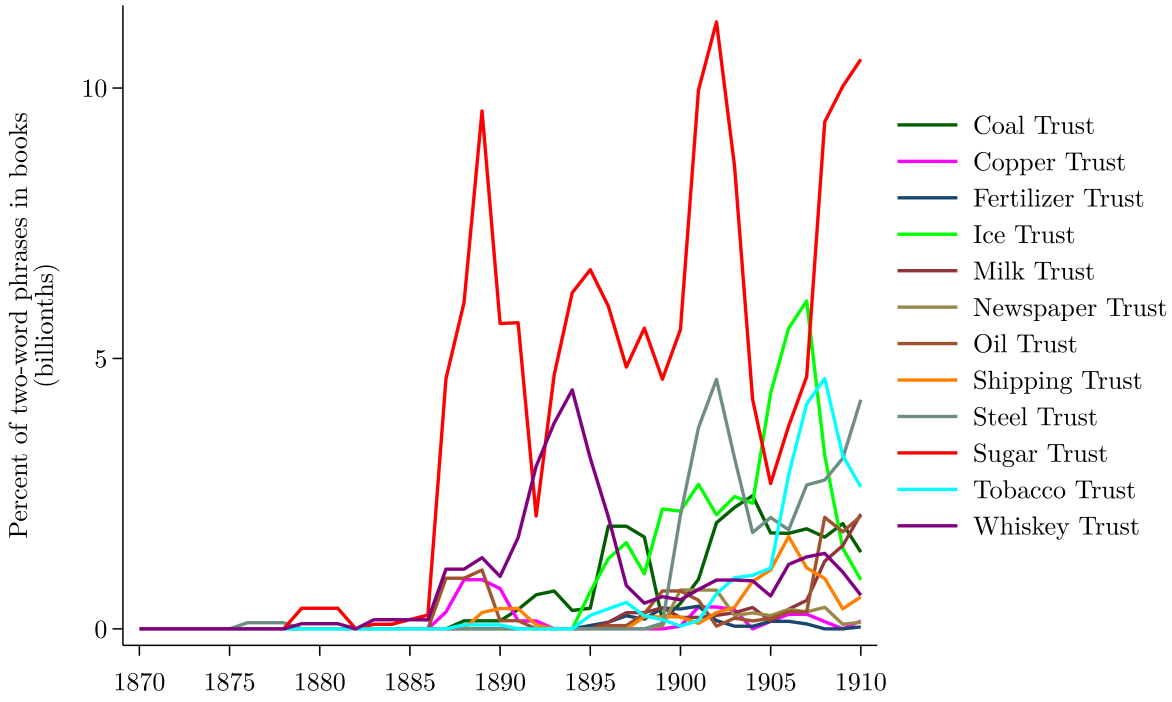
Figures

Figure 1: Mentions of Well-Known Trusts in the *Google Books* Corpus



Notes: This graph shows the percentage of two-word phrases in the *Google Books* corpus equal to each of the two-word trust names listed in the legend, by publication year. I obtained the underlying data from the Google Ngram Viewer (Michel et al. 2011).

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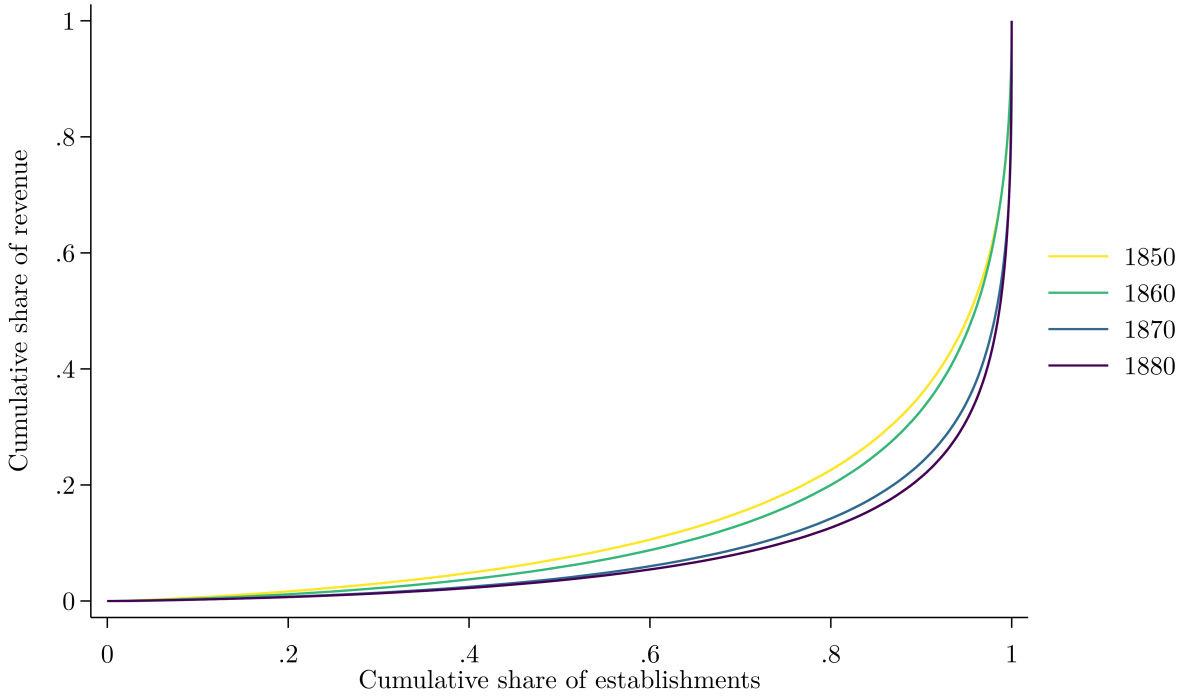
Figure 2: Concentration of Revenue in Manufacturing, 1850-1880



Notes: I estimated these Lorenz curves using the surviving schedules from the 1850-1880 censuses of manufactures (Hornbeck et al. 2024). Schedules for 1880 are missing from industries in which special agents were appointed to collect manufacturing data (Atack and Bateman 1999; Delle Donne 1973). These industries are iron and steel, cotton goods, woolen and worsted goods, silk and silk goods, chemical products and salt, coke and glass, shipbuilding, fisheries, all types of mining, coal, and petroleum (Wright 1900, p. 63). For consistency over time, I removed these industries in estimating the Lorenz curves shown above.

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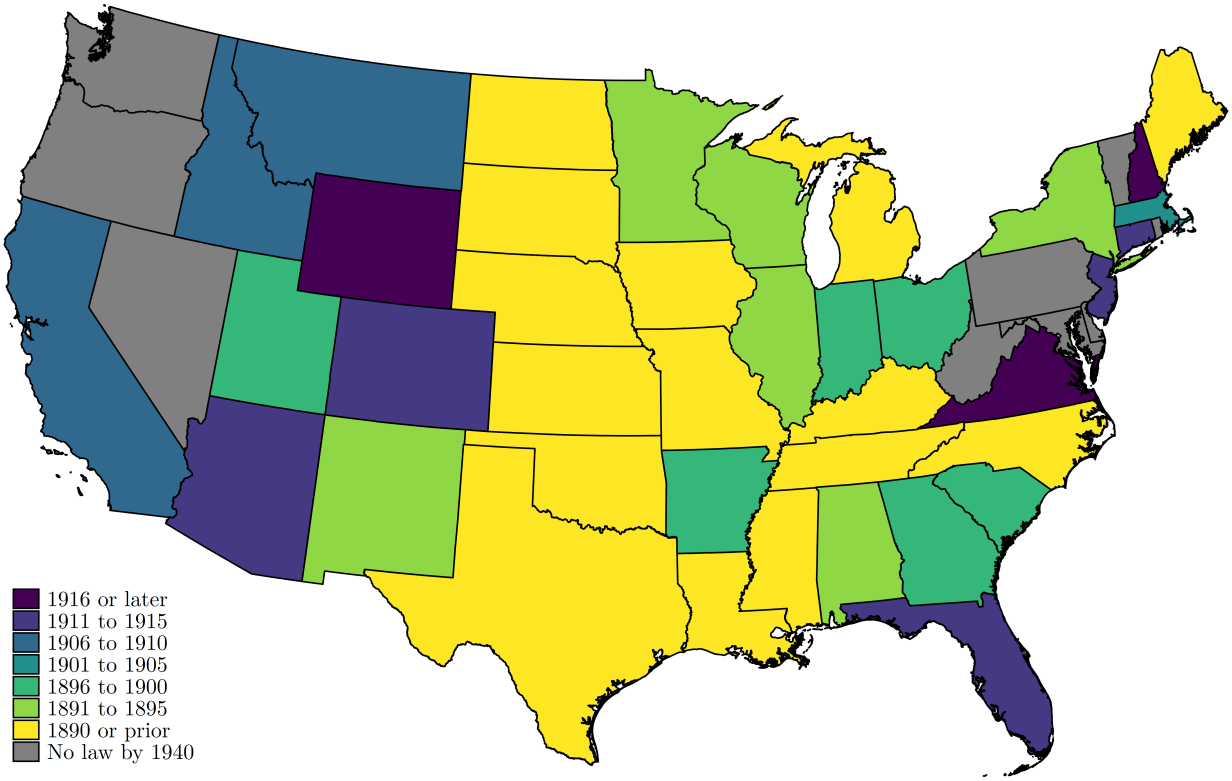
Figure 3: Map of State Antitrust Law Adoption



Notes: This map is shaded to show the year in which a state first adopted an antitrust law. In some cases, laws were later repealed by legislative act or overturned by court ruling, but this map does not reflect those changes. Thirteen states had already adopted an antitrust statute of their own by the time the Sherman Act—the first federal antitrust law—was enacted in 1890. These are the states shaded in yellow, excepting Oklahoma and Louisiana, which enacted antitrust statutes in 1890 after the passage of the Sherman Act on July 2, 1890. State boundaries in 1940 are shown.

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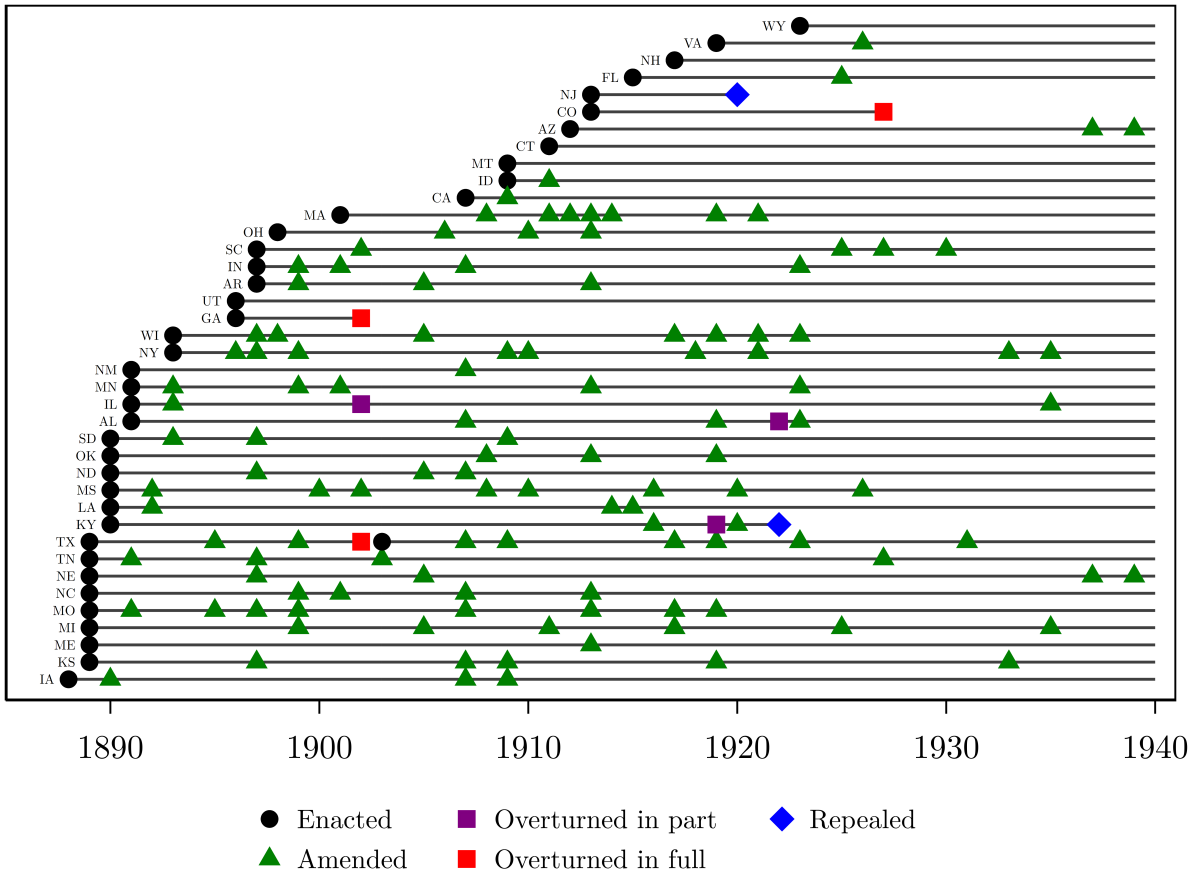
Figure 4: State Antitrust Statutes, 1888-1940



Notes: This figure illustrates the years in which state legislatures adopted and amended antitrust statutes, as well as the years in which antitrust statutes were repealed by legislative act or overturned in full or in part by a court. Delaware, Maryland, Nevada, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, and West Virginia are not included in this figure because these states did not enact a general antitrust statute between 1860 and 1940. Alaska and Hawaii are not included in this figure because these states achieved statehood after 1940.

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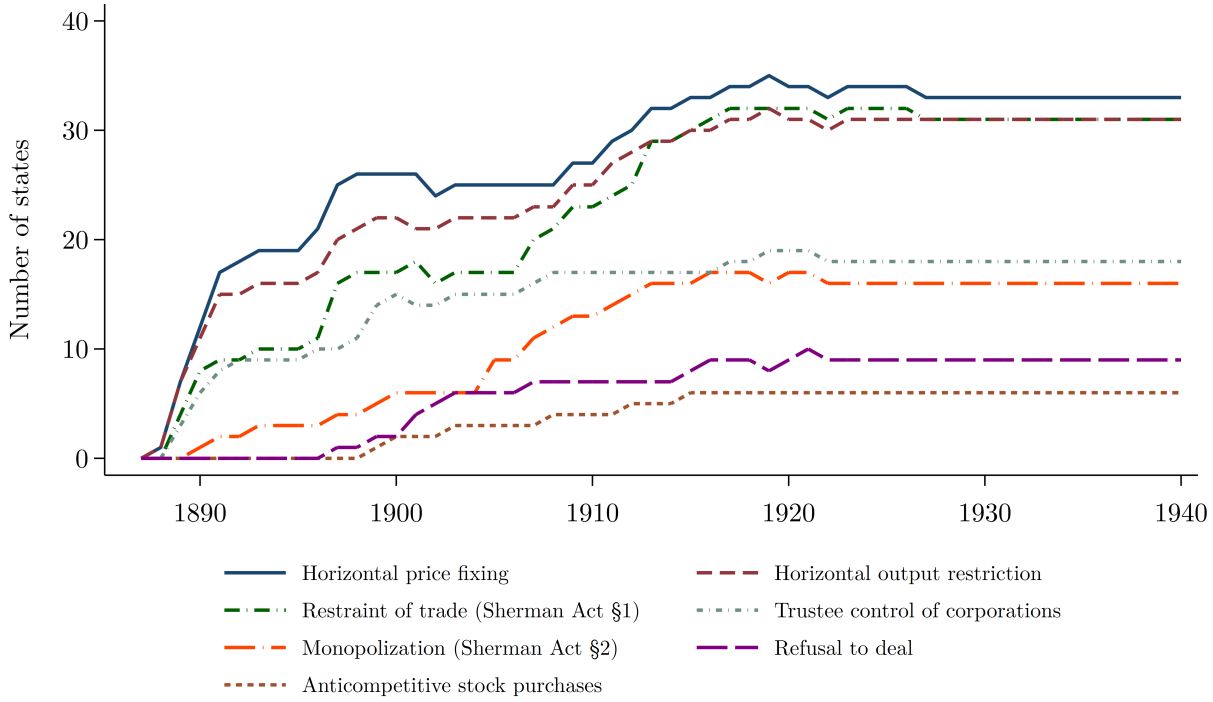
Figure 5: Anticompetitive Acts Made Illegal by State Antitrust Laws, 1888-1940



Notes: This figure illustrates the number of states that outlawed various anticompetitve acts between 1888 and 1940. See Table B1 for definitions of these acts.

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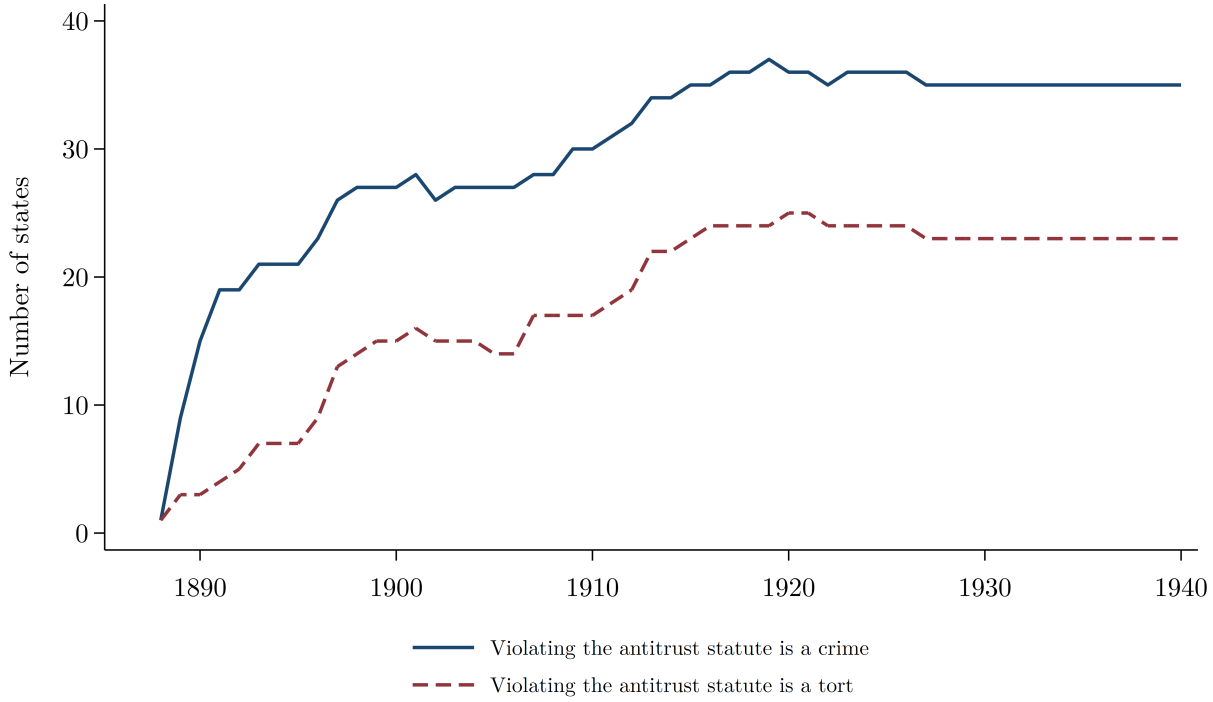
Figure 6: States Declaring Antitrust Violations Crimes vs. Torts, 1888-1940



Notes: This figure illustrates the number of states that declared violations of their antitrust statute to be crimes and the number of states that declared violations of their antitrust statute to be torts between 1888 and 1940. A crime is a wrongful act that violates a law and is prosecuted by the government, while a tort is a civil wrong that causes harm to an individual, who may seek compensation through a lawsuit.

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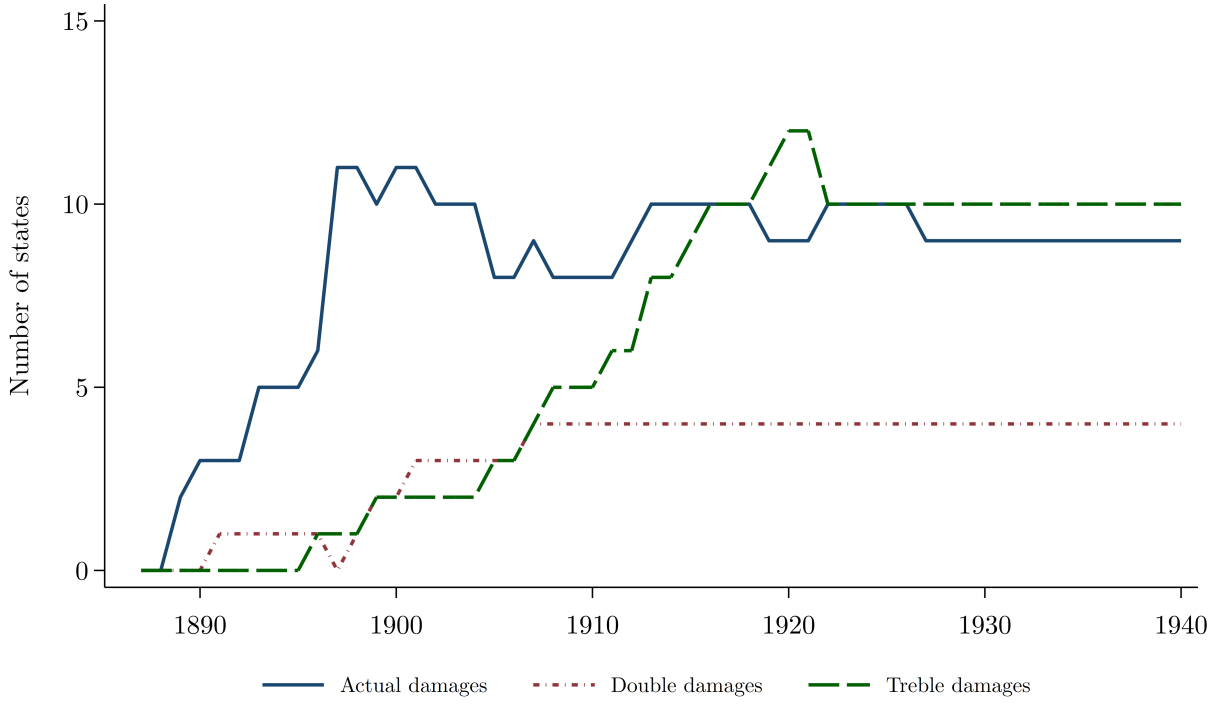
Figure 7: Civil Damages Under State Antitrust Laws, 1888-1940



Notes: This figure illustrates the number of states authorizing plaintiffs to seek actual damages, double damages, or treble damages in civil actions for injury due to unlawful conduct under state antitrust statutes.

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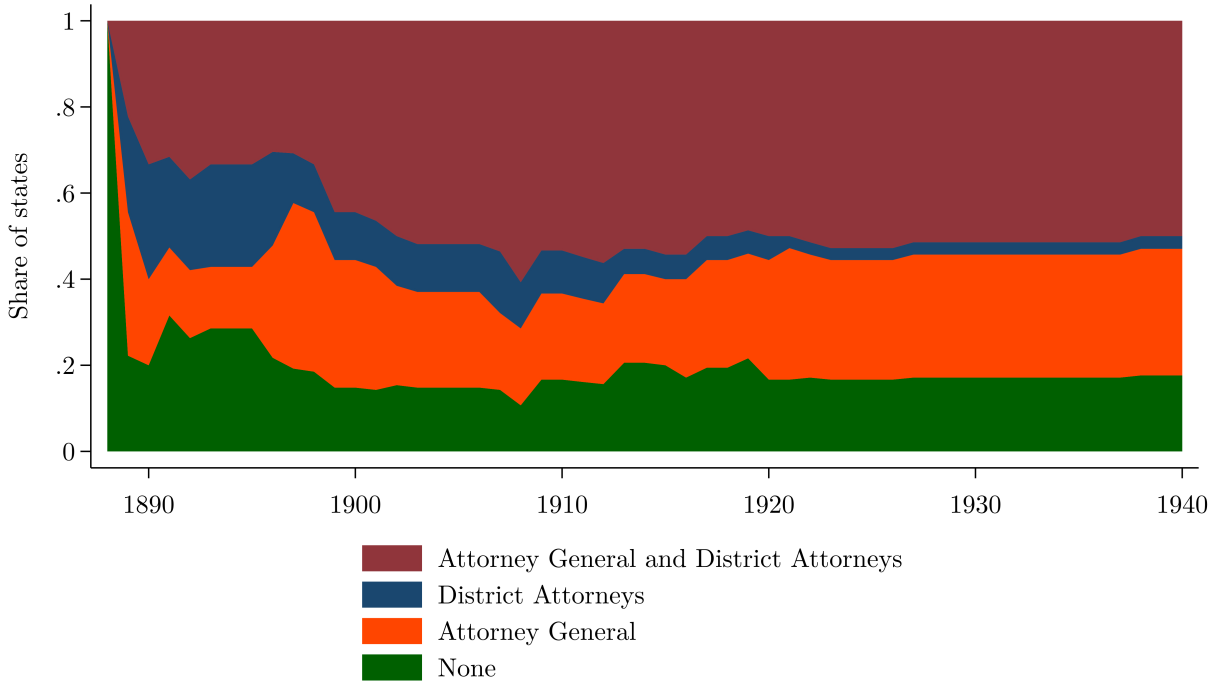
Figure 8: Enforcement Duties Under State Antitrust Laws, 1888-1940



Notes: This figure illustrates the share of states designating the duty to enforce the antitrust statute to both the Attorney General and District Attorneys, District Attorneys only, the Attorney General only, or no authority at all. Shares are calculated among states with an antitrust statute in force in each year.

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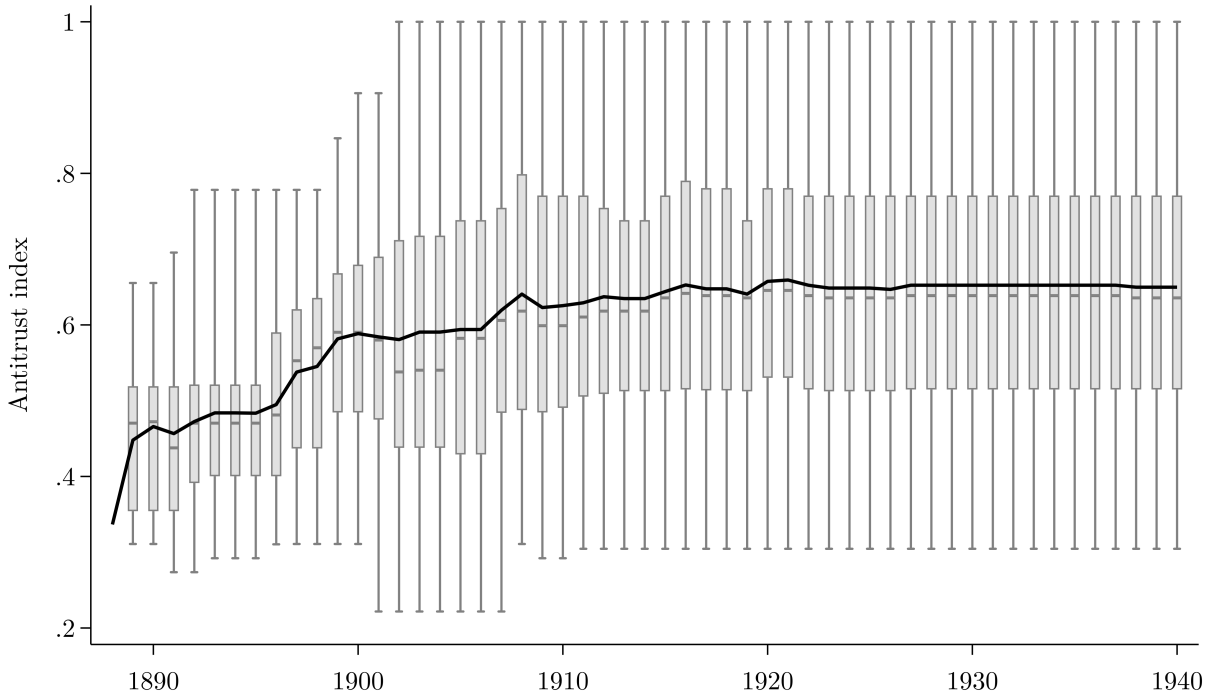
Figure 9: Antitrust Law Index, 1888-1940



Notes: This figure illustrates the distribution of my antitrust law index over time, among states that had an antitrust statue by 1940. The index is constructed using the first principal component from a principal component analysis of statutory features, rescaled to range from 0 to 1. Section 3 describes the construction of the index in greater detail. Table A1 details the variables I used to construct my antitrust law index and the weight assigned to each one. The black line indicates the annual mean across states, while the boxplots show the distribution in each year (interquartile range in gray boxes, median denoted by a gray line within each gray box, and whiskers extending to the minimum and maximum values). The sample includes all U.S. states except Alaska and Hawaii. In 1888, only Iowa had an antitrust statute, so no distribution is shown for that year.

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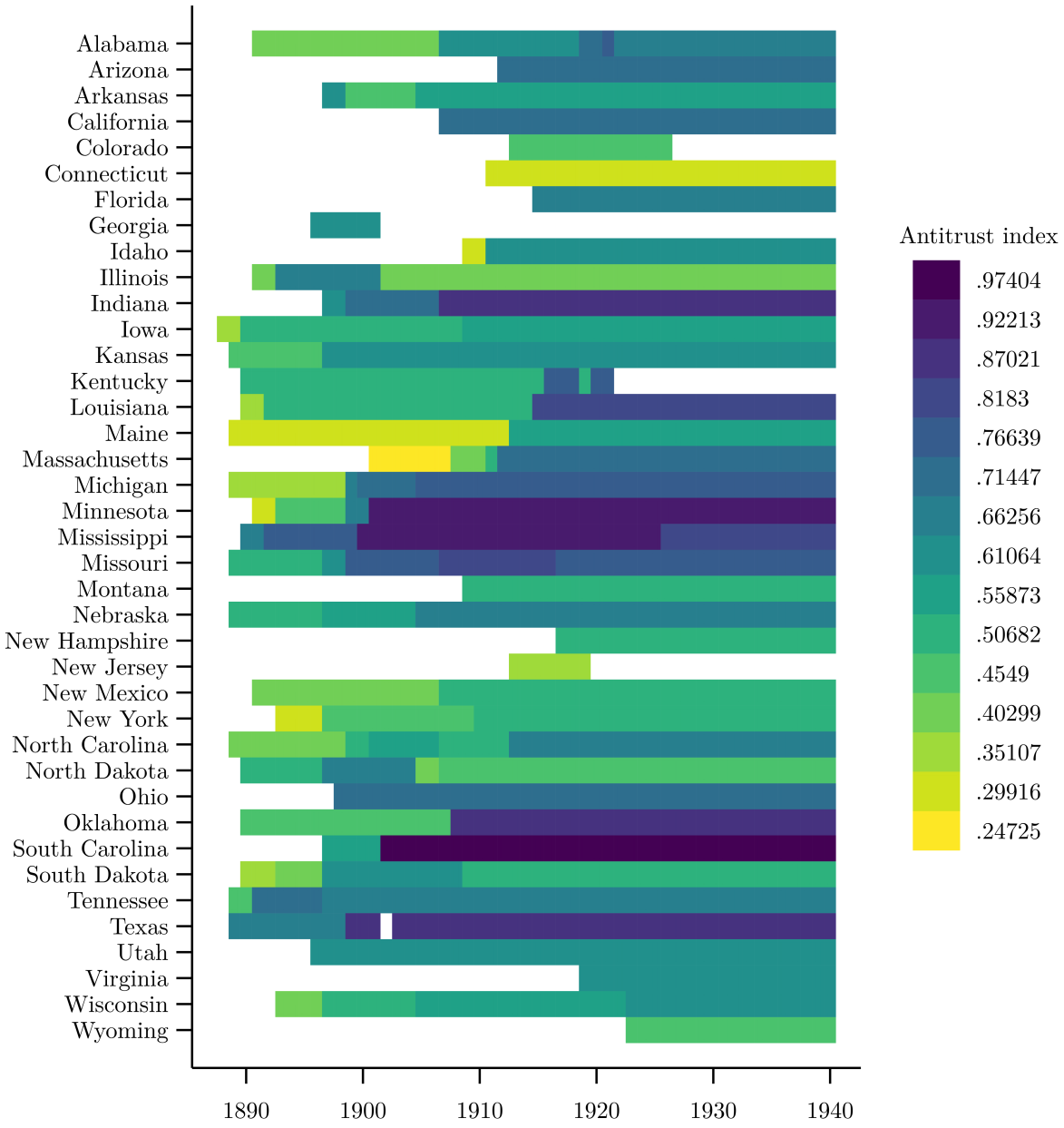
Figure 10: Antitrust Law Index by State, 1888-1940



Notes: This figure illustrates the antitrust law index over time, by state, among states that had an antitrust statue. The index is constructed using the first principal component from a principal component analysis of statutory features, rescaled to range from 0 to 1. Section 3 describes the construction of the index in greater detail. Table A1 details the variables I used to construct my antitrust law index and the weight assigned to each one. The sample includes all U.S. states except Alaska and Hawaii.

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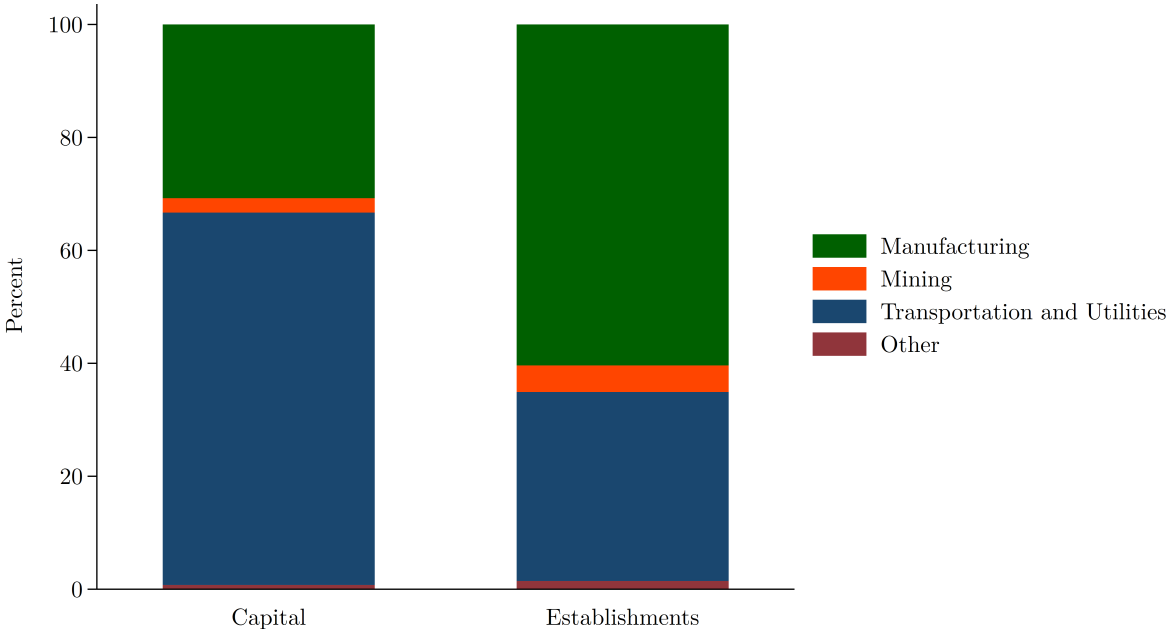
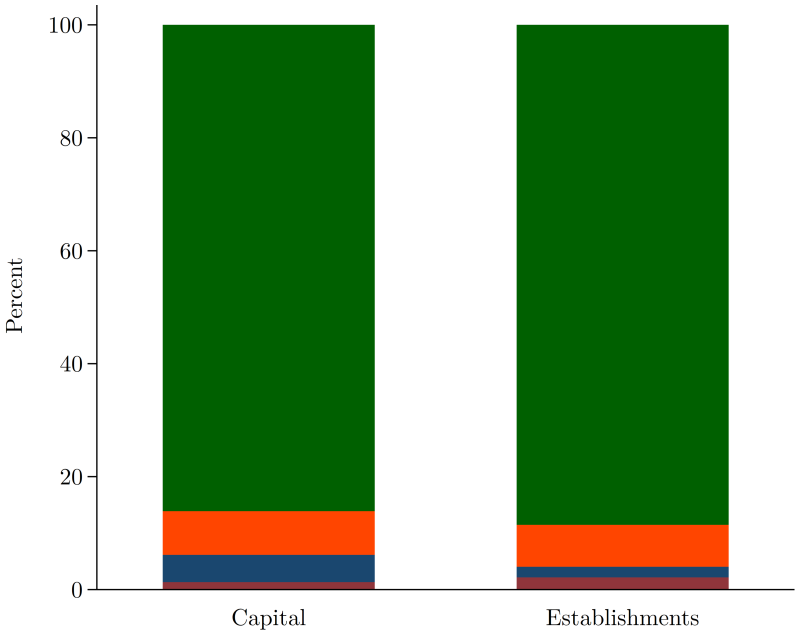
Figure 11: Trusts by Sector, 1903

Panel A: All Trusts



Panel B: "Industrial" Trusts



Notes: This figure shows the distribution of capital and establishments across the trusts listed in Moody (1904). These distributions are given for all 445 trusts in Panel A and for the 305 "industrial" trusts in Panel B.

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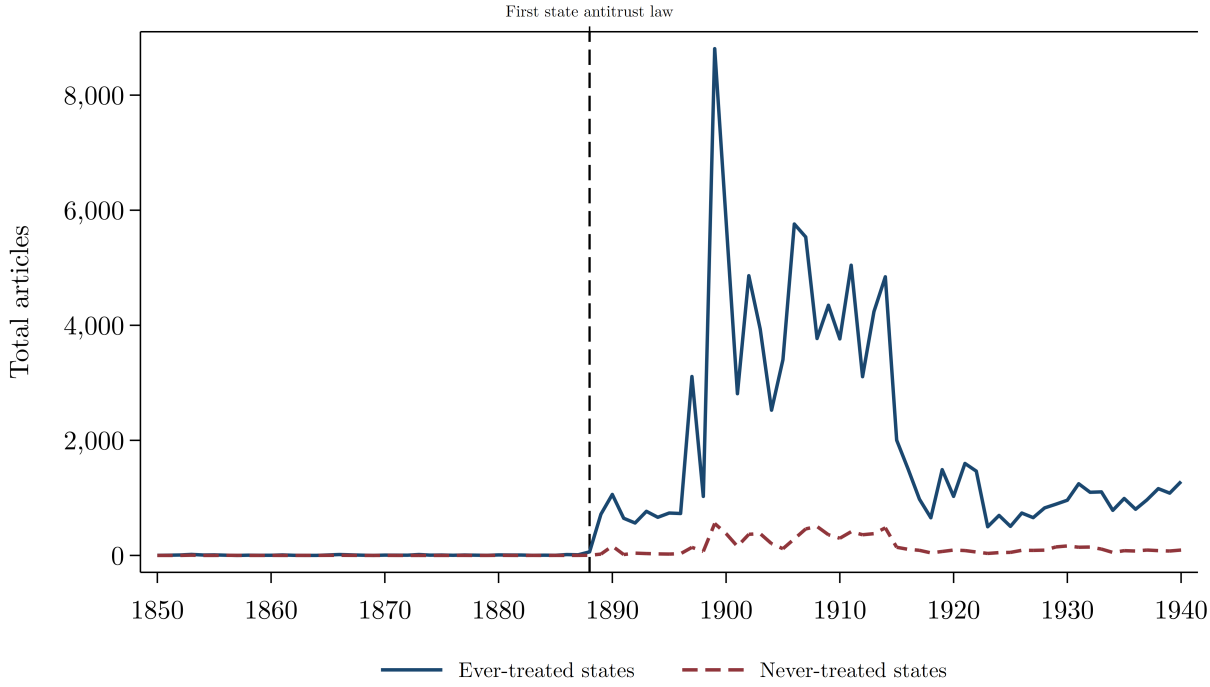
Figure 12: Total Number of Newspaper Articles Mentioning State Antitrust Laws



Notes: "Ever-treated" states adopted a state antitrust statute at some point between 1888 and 1940. "Never-treated" states did not adopt a state antitrust statute at any point during this period.

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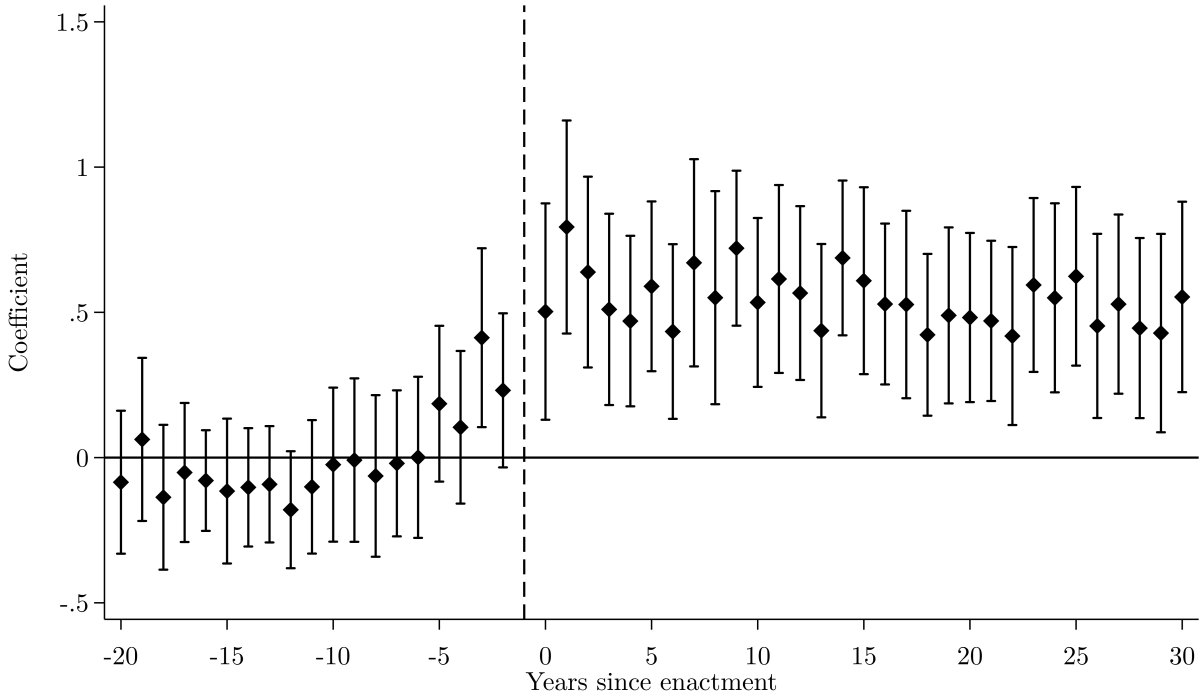
Figure 13: Effect of Enactment on the Number of Newspaper Articles Mentioning State Antitrust Laws



Notes: The dependent variable is the log number of newspaper articles mentioning each state's antitrust law. Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. This event study does not include controls.

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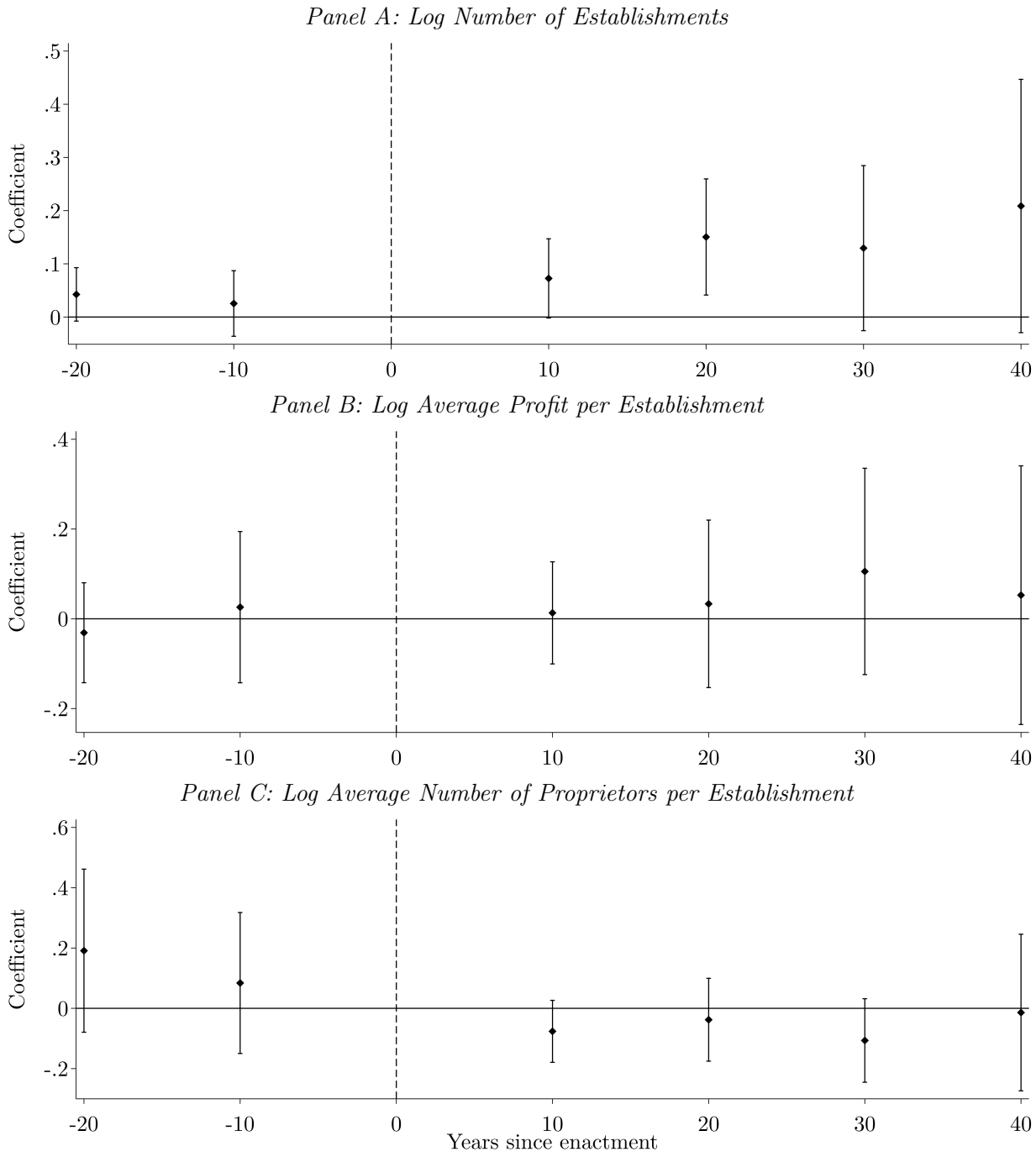
Figure 14: Event Study Results for Establishment Outcomes



Notes: Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

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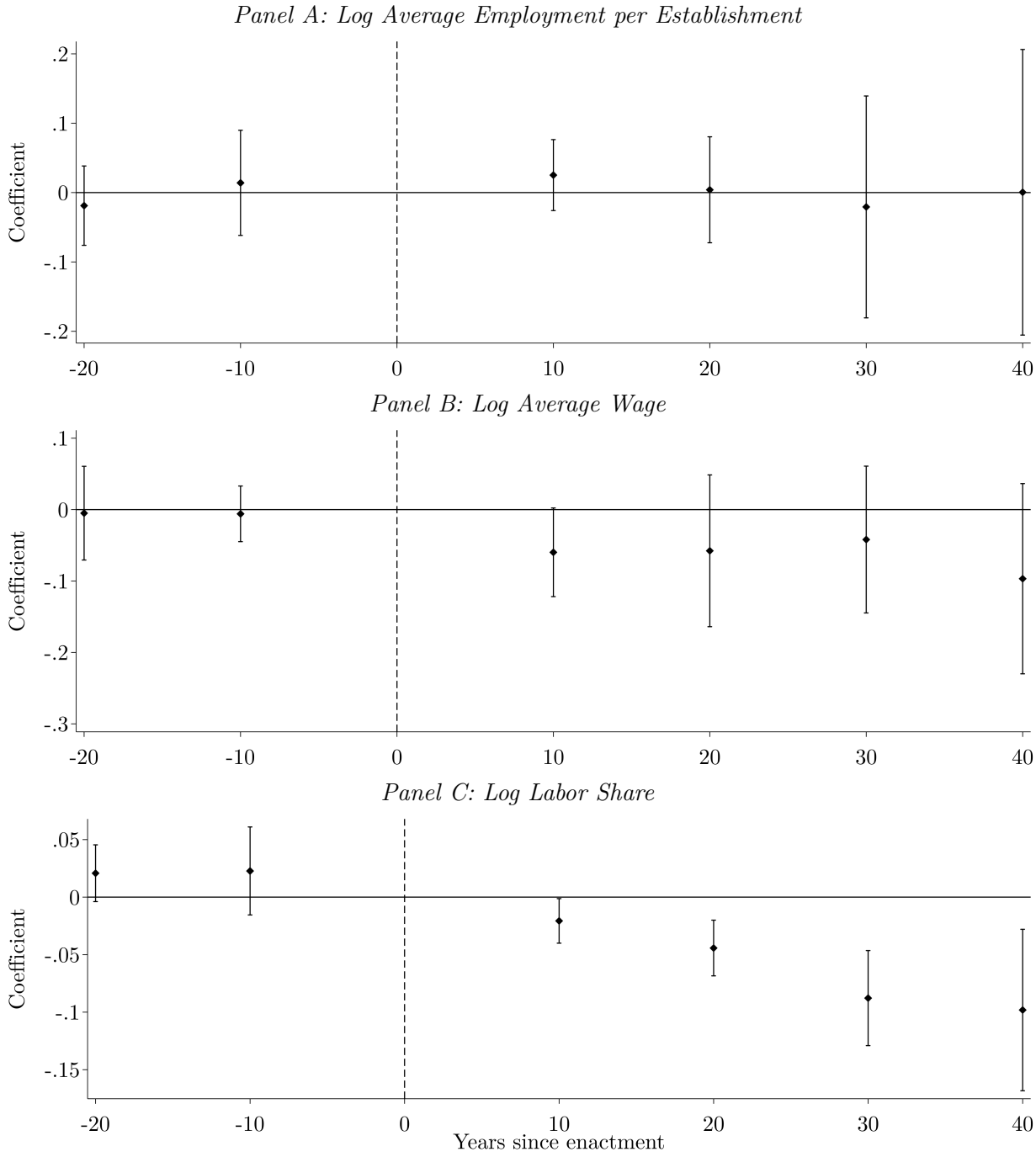
Figure 15: Event Study Results for Labor Market Outcomes



Notes: Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

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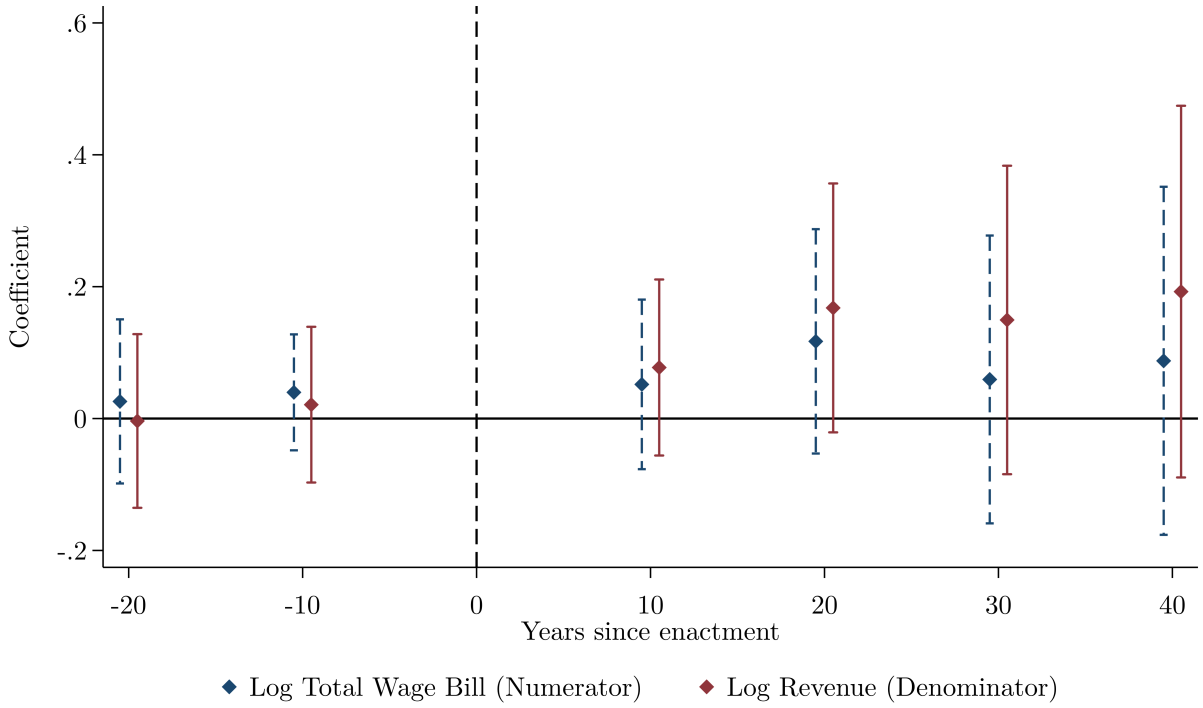
Figure 16: Event Study Results for Labor Share Components



Notes: Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

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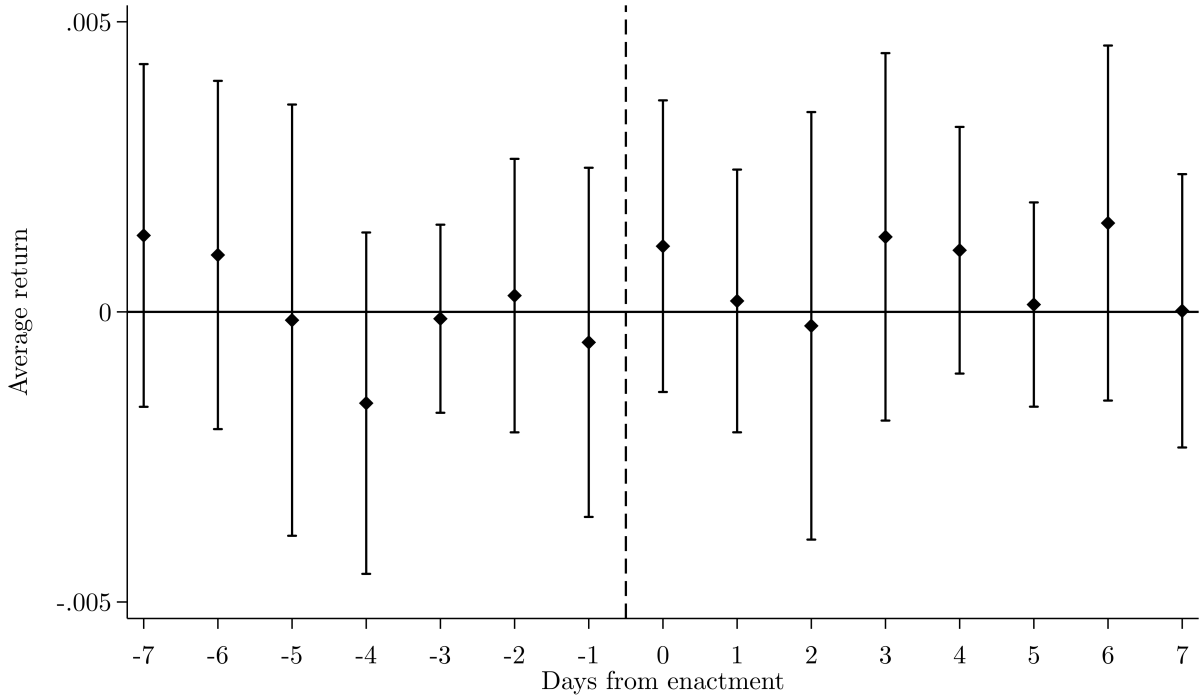
Figure 17: Average Daily Stock Returns Around Enactment



Notes: This figure shows the average daily return to the Dow Jones composite portfolio in the seven trading days before and after the passage of a state antitrust law. For each passage date, I extracted a 15-day event window (date of passage ± seven days) using historical daily return data (Schwert 1990). To account for non-trading days (i.e., weekends and holidays), I selected the 15 closest trading days around each date of passage. I then stacked the data across all dates of passage and computed the mean return and its standard error for each relative day. Days appearing in multiple states' windows were thus included multiple times. Bars indicate 95 percent confidence intervals.

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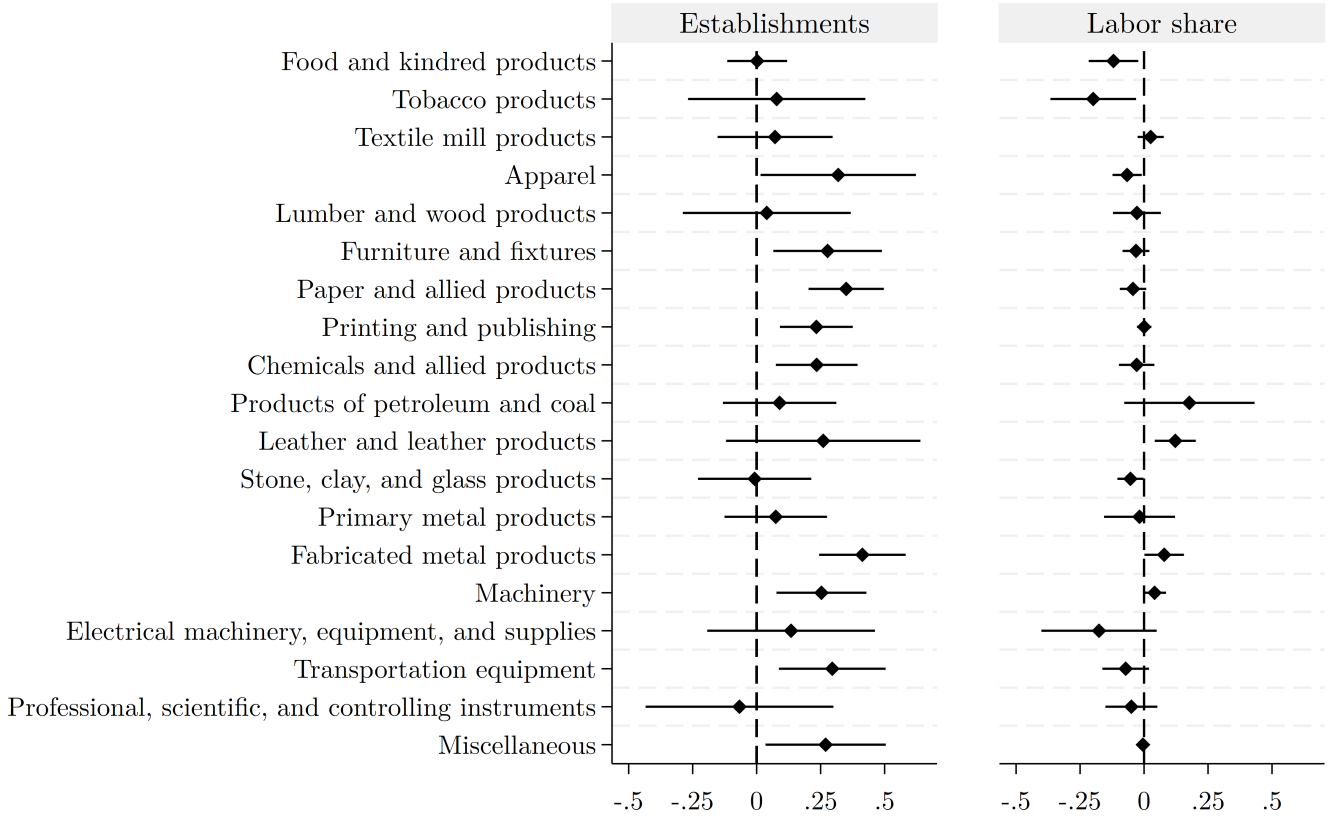
Figure 18: Difference-in-Differences Results by Industry Category



Notes: The dependent variable is the log number of manufacturing establishments in the first column and the log labor share in the second column. Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

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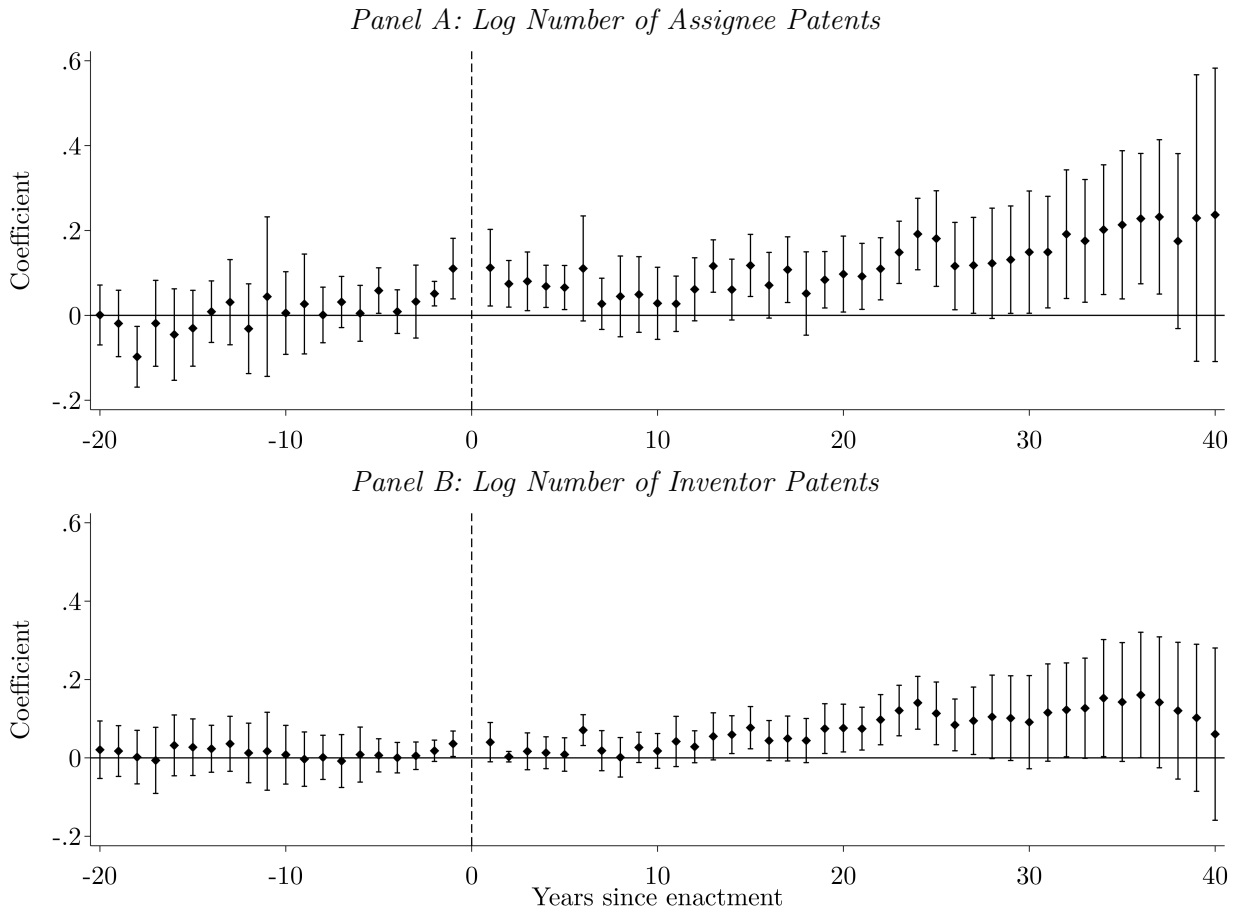
Figure 19: Event Study Results for Patenting Behavior



Notes: Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. The dependent variable is the number of patents, aggregated to the county-by–USPC–by-year level, where USPC denotes the U.S. Patent Classification (USPC) categories. This method of aggregation maximizes statistical power by retaining geographic and technological variation and enables the use of USPC category fixed effects, which allow me to account for systematic differences in patenting intensity across technologies. Regressions are weighted by the number of patents. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. USPC category fixed effects are also included.

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Tables

Table 1: Main Difference-in-Differences Results

	(1)	(2)	(3)	(4)
Panel A: Log Number of Establishments				
ATT	0.187	0.083	0.181**	0.102**
	(0.125)	(0.072)	(0.088)	(0.050)
Observations	70,322	70,322	70,322	70,322
Panel B: Log Average Profit per Establishment				
ATT	-0.051	-0.074	-0.040	-0.012
	(0.093)	(0.095)	(0.060)	(0.058)
Observations	68,801	68,801	68,801	68,801
Panel C: Log Average Number of Proprietors per Establishment				
ATT	-0.091	-0.111	-0.060	-0.098
	(0.150)	(0.146)	(0.124)	(0.121)
Observations	29,878	29,878	29,878	29,878
Panel D: Log Average Employment per Establishment				
ATT	-0.077	-0.089	-0.059	-0.002
	(0.065)	(0.069)	(0.037)	(0.037)
Observations	70,115	70,115	70,115	70,115
Panel E: Log Average Wage				
ATT	-0.026	-0.026	-0.040*	-0.034
	(0.024)	(0.024)	(0.022)	(0.021)
Observations	69,160	69,160	69,160	69,160
Panel F: Log Labor Share				
ATT	-0.048**	-0.039**	-0.053***	-0.042***
	(0.019)	(0.019)	(0.018)	(0.014)
Observations	69,158	69,158	69,158	69,158
Baseline controls	✓	✓	✓	✓
Industry FE	✓			
State-by-industry FE		✓		✓
Year-by-industry FE			✓	✓

Notes: Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level, and * denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state.

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Table 2: Difference-in-Differences Results by Trust Status

	Industries with a Trust		Industries with No Trust	
	(1)	(2)	(3)	(4)
Panel A: Log Number of Establishments				
ATT	0.127	0.061	0.211*	0.155**
	(0.088)	(0.053)	(0.105)	(0.066)
Observations	30,807	30,807	39,515	39,515
Panel B: Log Average Profit per Establishment				
ATT	0.014	0.003	-0.069	-0.020
	(0.081)	(0.077)	(0.050)	(0.039)
Observations	30,144	30,144	38,657	38,657
Panel C: Log Average Number of Proprietors per Establishment				
ATT	0.028	-0.011	-0.147*	-0.167*
	(0.156)	(0.139)	(0.086)	(0.088)
Observations	12,586	12,586	17,292	17,292
Panel D: Log Average Employment per Establishment				
ATT	-0.019	0.001	-0.062	0.011
	(0.046)	(0.050)	(0.053)	(0.033)
Observations	30,712	30,712	39,403	39,403
Panel E: Log Average Wage				
ATT	-0.047*	-0.033	-0.027	-0.031
	(0.024)	(0.022)	(0.024)	(0.025)
Observations	30,324	30,324	38,836	38,836
Panel F: Log Labor Share				
ATT	-0.071***	-0.053***	-0.025	-0.022
	(0.025)	(0.019)	(0.018)	(0.017)
Observations	30,322	30,322	38,836	38,836
Baseline controls	✓	✓	✓	✓
State-by-industry FE		✓		✓
Year-by-industry FE	✓	✓	✓	✓

Notes: Trust status is measured in 1903 and is from Moody 1904. Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level, and * denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state.

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Table 3: Difference-in-Differences Results by Level of Enforcement

	Low-Enforcement States		High-Enforcement States	
	(1)	(2)	(3)	(4)
Panel A: Log Number of Establishments				
ATT	0.203**	0.137*	0.220**	0.093
	(0.098)	(0.071)	(0.100)	(0.065)
Observations	43,033	43,033	43,272	43,272
Panel B: Log Average Profit per Establishment				
ATT	-0.040	-0.004	-0.025	0.002
	(0.124)	(0.123)	(0.077)	(0.079)
Observations	42,019	42,019	42,420	42,420
Panel C: Log Average Number of Proprietors per Establishment				
ATT	-0.116*	-0.175***	0.144	0.147
	(0.067)	(0.057)	(0.111)	(0.098)
Observations	17,787	17,787	18,176	18,176
Panel D: Log Average Employment per Establishment				
ATT	-0.030	0.048	-0.067	-0.017
	(0.085)	(0.085)	(0.043)	(0.055)
Observations	42,905	42,905	43,143	43,143
Panel E: Log Average Wage				
ATT	-0.061	-0.051	-0.048	-0.040
	(0.042)	(0.041)	(0.031)	(0.032)
Observations	42,268	42,268	42,614	42,614
Panel F: Log Labor Share				
ATT	-0.060**	-0.049*	-0.067**	-0.054***
	(0.025)	(0.025)	(0.027)	(0.019)
Observations	42,266	42,266	42,612	42,612
Baseline controls	✓	✓	✓	✓
State-by-industry FE		✓		✓
Year-by-industry FE	✓	✓	✓	✓

Notes: Enforcement is measured using mentions of state antitrust policy in contemporaneous newspapers. A state is designated as high enforcement if, during the years its antitrust statute was in effect, the share of years with above-median newspaper mentions per capita (relative to other treated states in that year) is at least as large as the median share across states. Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level, and * denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state.

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Table 4: Difference-in-Differences Results with Continuous Treatment

	(1)	(2)	Observations
Panel A: Log Number of Establishments			64,080
ATT	0.641***		
	(0.061)		
Baseline ATT (Projected Constant)		0.663***	
		(0.160)	
Antitrust Index		-0.036	
		(0.305)	
Panel B: Log Average Profit per Establishment			63,508
ATT	0.153		
	(0.133)		
Baseline ATT (Projected Constant)		-1.021**	
		(0.405)	
Antitrust Index		1.975***	
		(0.735)	
Panel C: Log Average Employment per Establishment			63,932
ATT	0.119		
	(0.111)		
Baseline ATT (Projected Constant)		-0.715**	
		(0.330)	
Antitrust Index		1.404***	
		(0.531)	
Panel D: Log Average Wage			63,766
ATT	-0.030		
	(0.034)		
Baseline ATT (Projected Constant)		-0.111	
		(0.080)	
Antitrust Index		0.136	
		(0.150)	
Panel E: Log Labor Share			63,770
ATT	-0.128***		
	(0.024)		
Baseline ATT (Projected Constant)		0.158**	
		(0.073)	
Antitrust Index		-0.480***	
		(0.124)	
Baseline controls	✓	✓	
State-by-industry FE	✓	✓	

Notes: Estimates are obtained using the Borusyak, Jaravel, and Spiess (2024) method. Robust standard errors clustered at the state level are reported in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level, and * denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state.

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Table 5: Robustness Checks on Main Difference-in-Differences Results

	(1)	(2)	(3)	(4)
Panel A: Log Number of Establishments				
ATT	0.148**	0.148**	0.100*	0.092*
	(0.065)	(0.062)	(0.051)	(0.051)
Observations	70,322	70,322	70,322	70,322
Panel B: Log Average Profit per Establishment				
ATT	-0.010	0.008	-0.015	-0.061
	(0.040)	(0.049)	(0.058)	(0.062)
Observations	68,801	68,801	68,801	68,801
Panel C: Log Average Number of Proprietors per Establishment				
ATT	-0.023	-0.140	-0.098	-0.130
	(0.073)	(0.103)	(0.121)	(0.112)
Observations	29,878	29,878	29,878	29,878
Panel D: Log Average Employment per Establishment				
ATT	-0.001	-0.013	-0.006	-0.026
	(0.035)	(0.041)	(0.037)	(0.038)
Observations	70,115	70,115	70,115	70,115
Panel E: Log Average Wage				
ATT	-0.018	-0.014	-0.032	-0.053*
	(0.020)	(0.020)	(0.021)	(0.027)
Observations	69,160	69,160	69,160	69,160
Panel F: Log Labor Share				
ATT	-0.030**	-0.024*	-0.040***	-0.033**
	(0.014)	(0.012)	(0.014)	(0.013)
Observations	69,158	69,158	69,158	69,158
Baseline controls	✓	✓	✓	✓
State-by-industry FE	✓	✓	✓	✓
Year-by-industry FE	✓	✓	✓	✓
Establishment weighting	✓			
Employment weighting		✓		
Const. provision control			✓	
Ag. share control				✓

Notes: Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level, and * denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state. Additionally, column (3) controls for whether the state constitution contained an anti-monopoly provision, and column (4) controls for the share of employment in agriculture.

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Table 6: Difference-in-Differences Results for Firms' Patenting Behavior

	(1)	(2)	(3)	(4)
Panel A: Log Number of Assignee Patents				
ATT	0.076***	0.085***	0.088***	0.074***
	(0.026)	(0.028)	(0.031)	(0.025)
Observations	272,496	272,496	272,496	272,496
Panel B: Log Number of Inventor Patents				
ATT	0.052***	0.045***	0.058***	0.051***
	(0.014)	(0.012)	(0.018)	(0.014)
Observations	747,934	747,934	747,934	747,934
Baseline controls	✓	✓	✓	✓
USPC FE	✓	✓		✓
USPC-by-year FE			✓	
Const. provision control	✓			
Ag. share control		✓		

Notes: Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level, and * denotes statistical significance at the 10 percent level. The dependent variable is the number of patents, aggregated to the county-by–USPC–by-year level, where USPC denotes the U.S. Patent Classification (USPC) categories. This method of aggregation maximizes statistical power by retaining geographic and technological variation and enables the use of USPC category fixed effects, which allow me to account for systematic differences in patenting intensity across technologies. Regressions are weighted by the number of patents. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state. USPC category fixed effects are also included in columns (1), (2), and (4), and column (3) includes USPC-by-year fixed effects. Additionally, column (1) controls for whether the state constitution contained an anti-monopoly provision, and column (2) controls for the share of employment in agriculture.

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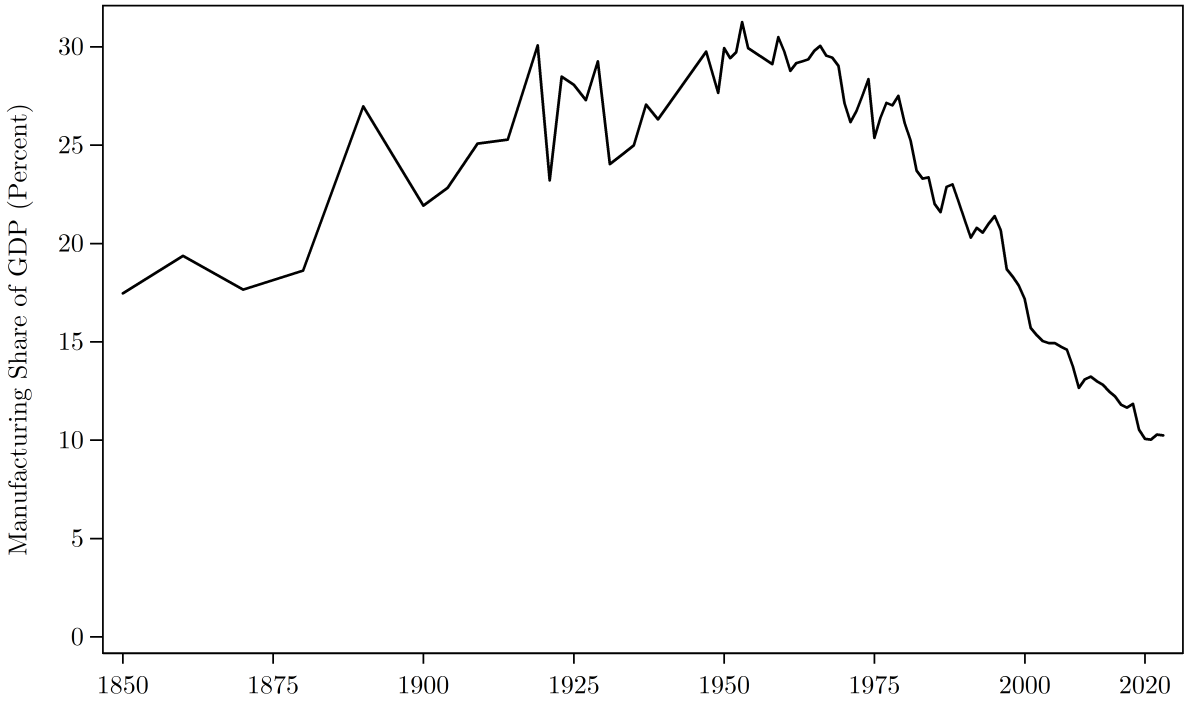
A Supplemental Figures and Tables

Figure A1: Manufacturing Share of Gross Domestic Product (GDP), 1850-2023



Notes: This figure plots the share of GDP in manufacturing, which is aggregate value added in the manufacturing sector over nominal GDP, for 1850 through 2023. I compiled data on value added in the manufacturing sector from several sources. The U.S. Bureau of the Census (1942, p. 19) provides data from past censuses of manufactures spanning 1850 through 1939 in Table 2. Between 1850 and 1890 so-called "hand and neighborhood" establishments are included in the data, but in 1900 and later years, the data exclude these smaller artisans and instead focus on larger factories. The U.S. Bureau of the Census (1957, p. 3) provides values for 1900 through 1954 in Table 1B. Becker, Gray, and Marvakov (2021) provide industry-level data for 1958 through 2018, which I aggregated by year. Finally, U.S. Bureau of Economic Analysis (2024) provide data for 1997 through 2023. In years where these sources overlap, I averaged their values, though values generally only differed slightly. GDP data are from Johnston and Williamson (2024) for 1850 to 2022 and Federal Reserve Bank of St. Louis (2024) for 2023. As Baily and Bosworth (2014) show, trends in manufacturing's share of *real* GDP differ from trends in manufacturing's share of *nominal* GDP because the prices of manufactured goods have increased at a different rate than the prices of non-manufactured goods. However, a separate price index for manufactured goods is not currently available going as far back as 1850, so the figure must be interpreted with this caveat in mind.

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Table A1: Antitrust Law Index Components

	Variable	First Principal Component	First Principal Component Squared
1	Violating the statute is a crime[1]	0.298	0.089
2	Violating the statute is a tort[1]	0.253	0.064
3	Minimum prison sentence for violating the statute[2]	0.148	0.022
4	Maximum prison sentence for violating the statute[2]	0.225	0.050
5	Minimum fine for violating the statute[2]	0.201	0.040
6	Maximum fine for violating the statute[2]	0.142	0.020
7	Level of damages that can be sought for violations[2]	0.231	0.053
8	The statute outlaws horizontal price fixing for goods[1]	0.291	0.085
9	The statute outlaws horizontal price fixing for services[1]	0.046	0.002
10	The statute outlaws horizontal output restriction[1]	0.275	0.075
11	The statute outlaws trustee control of corporations[1]	0.223	0.050
12	The statute outlaws restraints of trade[1]	0.278	0.077
13	The statute outlaws monopolization[1]	0.186	0.035
14	The statute outlaws anticompetitive stock purchases[1]	0.148	0.022
15	The statute outlaws refusals to deal[1]	0.167	0.028
16	The statute allows the state to revoke the charters of offending corporations[1]	0.275	0.076
17	The statute allows both a fine and a prison sentence to be imposed[2]	0.297	0.088
18	How the statute assigns enforcement responsibilities[2]	0.291	0.085
19	The statute stipulates that each day of violation is a separate offense[1]	0.198	0.039
	Total		**1.000**

Notes: "1" denotes an indicator variable and "2" denotes a continuous or ordinal variable. Minimum and maximum prison sentence variables (variables 3 and 4, respectively) record the minimum and maximum incarceration periods authorized for violating the antitrust statute. When no minimum prison sentence is set, I recode this variable to 0. I recode statutes with no statutory maximum (i.e., theoretically unlimited) to the longest maximum observed in the data. Minimum and maximum fine variables (variables 5 and 6, respectively) are constructed analogously. When no minimum fine is set, I recode this variable to 0, and when there is no statutory maximum, I recode to the highest maximum fine observed. The level of damages variable (variable 7) is coded as 1 for statutes authorizing actual damages, 2 for statutes authorizing double damages, and 3 for statutes authorizing treble damages. Variable 17 takes four values: 1 if the statute does not authorize either a fine or prison sentence, 2 if it authorizes one of the two but not both, 3 if it authorizes either or both, and 4 if it requires both a fine and a prison sentence. The enforcement responsibility variable (variable 18) takes four values: 1 if no official is assigned enforcement duties, 2 if only the Attorney General is assigned enforcement duties, 3 if all District Attorneys are assigned enforcement duties, and 4 if both the Attorney General and all District Attorneys are assigned enforcement duties.

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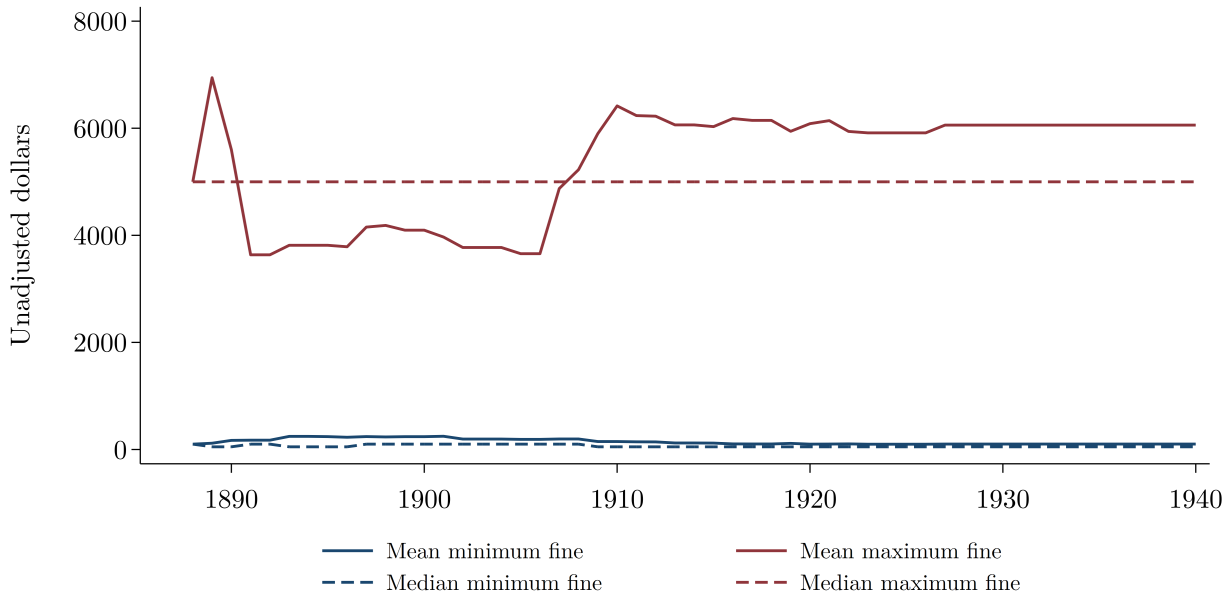
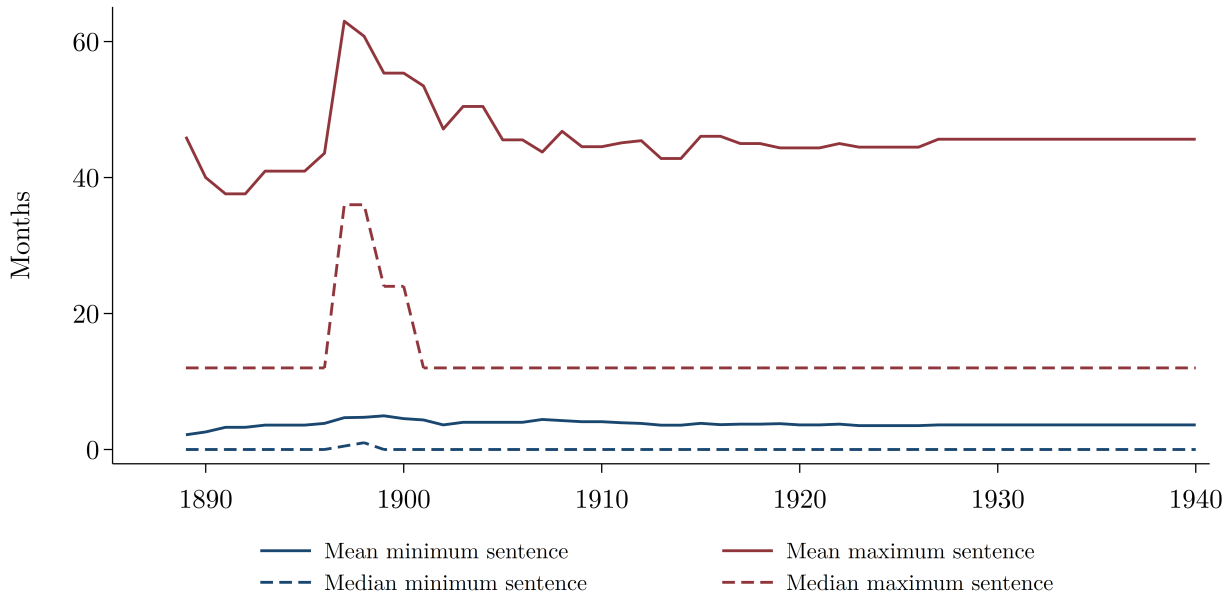
Figure A2: Fines and Prison Sentences Under State Antitrust Laws, 1888-1940

Panel A: Fines



Panel B: Prison Sentences



Notes: This figure illustrates the fines (Panel A) and prison sentences (Panel B) states described in their antitrust statutes. Among states with statutory minimum and maximum fines, mean and median values of these minimum and maximum fines are provided in unadjusted dollars in Panel A. Among states with statutory minimum and maximum prison sentences, mean and median values of these sentences are provided in months in Panel B.

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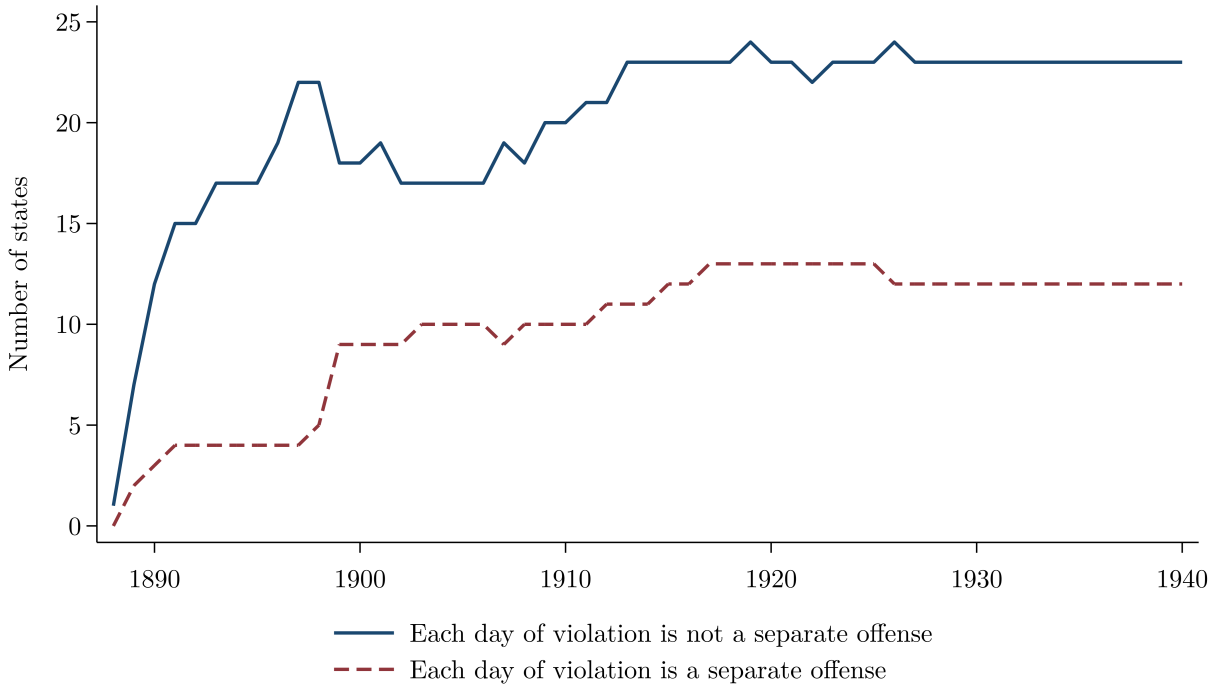
Figure A3: Provisions Making Each Day of Violation a Separate Offense Under State Antitrust Laws, 1888-1940



Notes: This figure illustrates the number of states with antitrust provisions specifying that each day of a continuing violation would constitute a separate offense.

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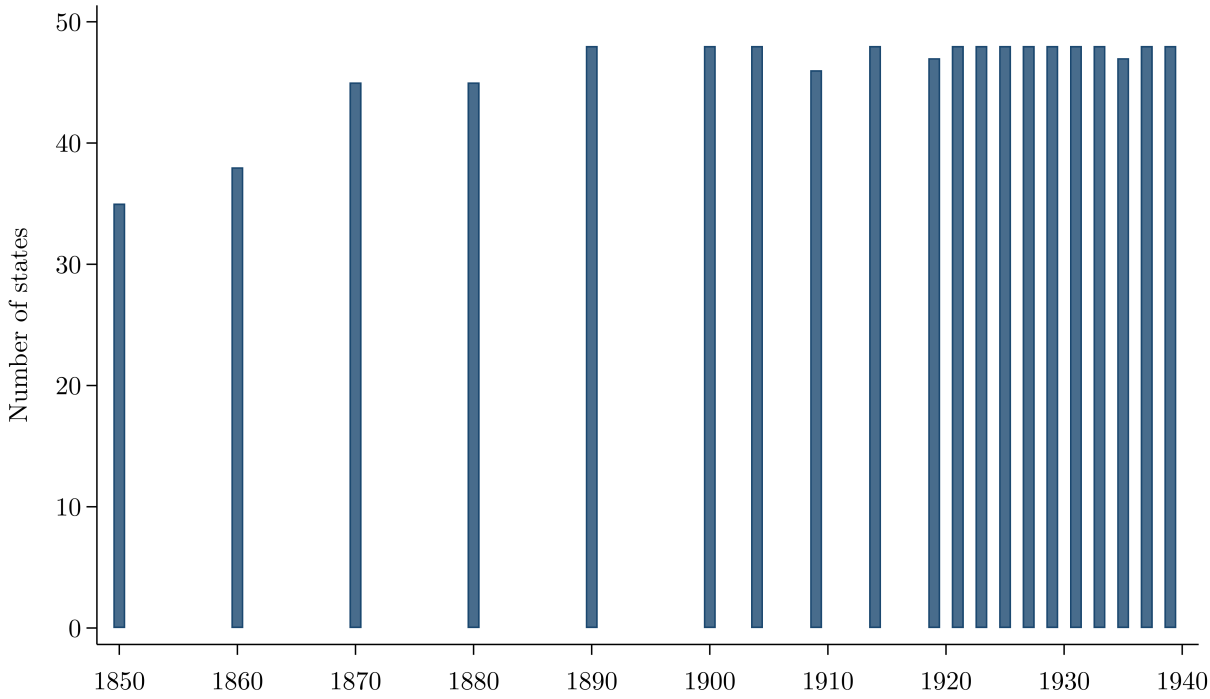
Figure A4: Coverage of State-by-Industry Census of Manufactures Data, 1850-1940



Notes: This figure illustrates the number of U.S. states included in the state-by-industry census of manufactures data from 1850 to 1940. Each bar corresponds to a census.

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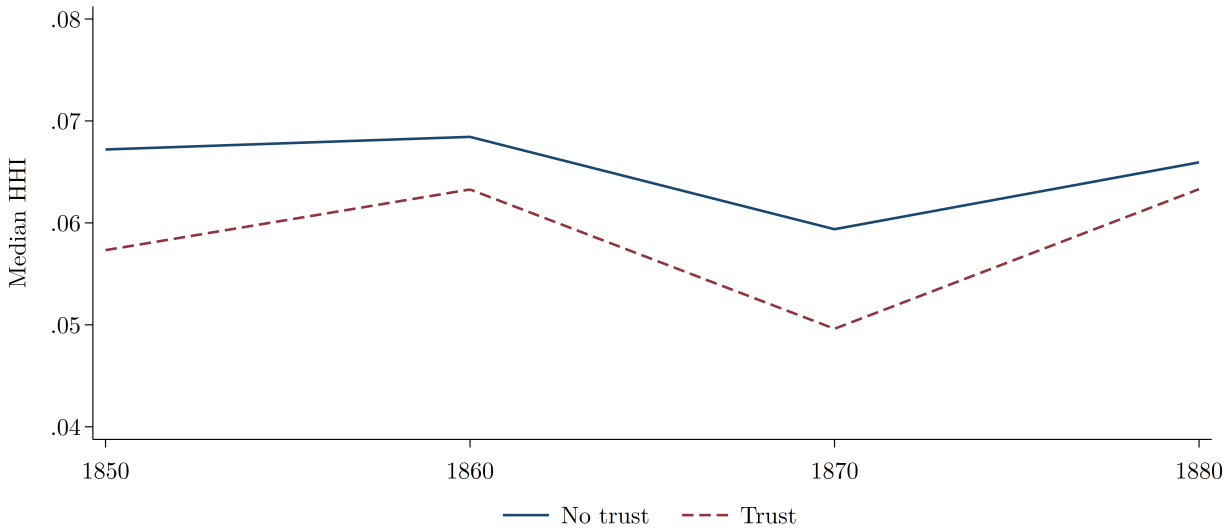
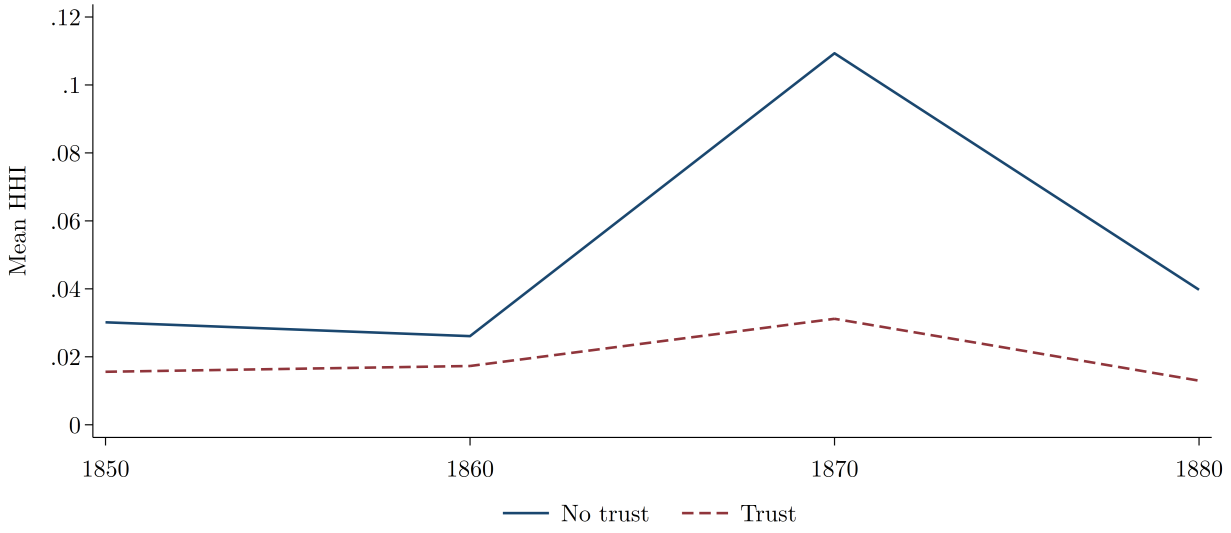
Figure A5: Establishment Concentration in Trust-Affiliated and Non-Trust Affiliated Industries, 1850-1880

Panel A: Unweighted Median HHI



Panel B: Revenue-Weighted Mean HHI



Notes: This figure illustrates the median (Panel A) and mean (Panel B) establishment-level Herfindahl–Hirschman Index (HHI) by trust status from 1850 through 1880. Panel A is unweighted, while Panel B weights industries by revenue. Trust status is measured in 1903 and is from Moody 1904. Schedules for 1880 are missing from industries in which special agents were appointed to collect manufacturing data (Atack and Bateman 1999; Delle Donne 1973). These industries are iron and steel, cotton goods, woolen and worsted goods, silk and silk goods, chemical products and salt, coke and glass, shipbuilding, fisheries, all types of mining, coal, and petroleum (Wright 1900, p. 63). For consistency over time, I removed these industries in the graphs shown above.

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Figure A6: Examples of Newspaper Articles Mentioning State Antitrust Laws

IOWA'S ANTI-TRUST LAW.

NO PROPER MACHINERY PROVIDED FOR ITS ENFORCEMENT.

DES MOINES, Iowa, June 6.—The Attorney General has given an important opinion in regard to the anti-trust law of the last General Assembly. That law provides that the Secretary of State shall address inquiries to all the corporations organized under the laws of or doing business in the State, as to whether they are violating the provisions of the act by unlawfully combining or merging their corporate existence in any other company. These inquiries the proper officer of the company is compelled to answer under oath. In event of their refusal to comply with the requirements of the statute, the Attorney General, through any County Attorneys of the State, shall direct proceedings, which shall have as their result on conviction, the revocation of the charter of the company.

The late Secretary Jackson sent the proper form of affidavit to the companies of the State. The results obtained can be tabulated: Corporations complying, 23; corporations as to which the Secretary had information that they had gone out of business, 370; corporations to which letters addressed were returned marked "Uncalled for," 951; corporations from which no reply was received, 1,991. These returns were properly certified to the Attorney General. Before instituting suits he concluded to examine the evidence obtained. As to the first three classes, there is no evidence that they have "refused" to comply with the demands of the State. Large numbers of them are agricultural, charitable, religious, and educational in character, and it is likely that many of the communications did not reach them or reached other parties who might formerly have been officers.

The Attorney General also maintains that it was hardly the intention of the General Assembly that summary measures should be instituted against corporations of this character. After discussing the difficulties of the situation, and what would be proper evidence to enable him to bring suit with any degree of expectancy of success, the Attorney General concludes: "In view of the insufficiency of the machinery provided for the execution of this section of this statute, I think the situation is of so grave a character as to warrant us in calling the attention of the Governor to it for such consideration as he may deem proper in the preparation of his biennial message to the General Assembly."

(a) *New York Times*
New York, New York
June 7, 1891

THE GEORGIA ANTI-TRUST LAW.

(Atlanta Journal.)

The Journal has received from many States requests for copies of the Georgia anti-trust law. In some instances these requests have come from members of Legislatures who stated that they intended to introduce anti-trust bills. The Georgia law has attracted especial attention because it proved so speedily effective. A bill that will stop the operations of trusts in a State has naturally attracted very general attention. The Georgia bill has been introduced in four or five Legislatures, and it is probable that several other States will soon have anti-trust laws copied after ours. The Alabama House of Representatives last Saturday, without a dissenting voice, passed a bill which copies the Georgia law exactly. It is said that it wll certainly pass the Senate and become a law at the present session.

The Georgia Legislature seems to have set the anti-trust ball rolling at a lively rate. Let 'er roll.

(b) *News and Observer*
Raleigh, North Carolina
February 10, 1897

STANDARD OIL FOUND GUILTY

Combine Convicted of Violating the Anti-Trust Law of Ohio.

Special Dispatch to the "Chronicle."

FINDLAY (O.), October 19.—By the verdict of a jury the Standard Oil Company of Ohio is guilty of conspiracy against trade, in violation of the Valentine anti-trust law of Ohio. The penalty is a fine of from $50 to $5000, which may be repeated for each day of the offense, or imprisonment of six to twelve months.

The Standard Oil Company of Ohio has given notice that it will file a motion for a new trial. Under the practice of the Court, the defendant has three days to put this motion in form. The next step will be for the Court to impose the penalty. The defense will then take their bill of exceptions to such rulings of Judge Banker as they objected to to the Circuit Court of the State. The appeal from this court is to the Supreme Court of the State, by which tribunal there is no doubt the issue will ultimately be decided.

To the State the suit, the verdict and the ultimate appeal is important, particularly because it initiates an entirely new method of proceeding against the alleged trade monopolies—that is, by information and affidavit, instead of by Grand Jury indictment.

The verdict was rendered at 4:30 o'clock this morning, and resulted from a continuous deliberation by the jury during thirty-two consecutive hours. The trial occupied seven days preceding this deliberation. When the case went to the jury at 8:30 o'clock Wednesday night the first ballot of the jurors stood nine for conviction and three for acquittal. As the result of continuous deliberations to 4 o'clock Thursday morning, one of the three for acquittal joined the majority. At 7 o'clock Thursday night one of the two remaining for acquittal went over to the other side, and at 4 o'clock this morning the last of the three gave his assent to the verdict of "guilty."

A touch of the dramatic marked the closing hours of the jury's deliberations. Hymns were sung during all but ten minutes of this time. This ten minutes came an the end and was occupied by the remaining juror who had stood out in explaining his position and surrender to the majority. There was not the slightest levity about this hymnal service. The jurors had then been many hours without sleep. The songs, which were started shortly after 2 o'clock in the morning by about three voices, echoed at first feebly through the spacious courthouse. After one familiar hymn after another was sung, it was evident that the spirit of fraternalism was gaining headway in the small chamber in which the twelve men were locked. The number of voices increased, the hymns gained in volume and enthusiasm. Then "Home, Sweet Home" was sung, the national anthem followed, then more hymns. Laughter was heard between the limited pauses. It bore no tone of derision, but of cordiality. A few minutes later came the announcement that the jury had reached an agreement and Judge Bankers hastily summoned.

(c) *San Francisco Chronicle*
San Francisco, California
October 20, 1906

ANTI-TRUST SUIT SEEKS MILLIONS

LEXINGTON, Miss.—A suit has been entered by the Retail Lumber Dealers Association of Mississippi and Louisiana in the chancery court under the anti-trust statute for the recovery of penalties aggregating $14,184,000, the minimum under the statute, as the minimum penalty is $200 a day and the maximum $500

(d) *Christian Science Monitor*
Boston, Massachusetts
July 16, 1909

MISSOURI COURT OUSTS OPEN PRICE BOARDS

St. Louis Lumber Exchange's Members Fined $96,000 Under Anti-Trust Laws.

JEFFERSON CITY, Mo., Aug. 31 (Associated Press).—Missouri has scored a complete victory over the new form of business combinations known as "open price associations," in a suit brought by Attorney General Barrett against the St. Louis Lumber Trade Exchange. The Missouri Supreme Court ousted each of the nineteen St. Louis lumber companies composing the exchange, assessed fines totaling $96,000 and ousted the exchange itself.

The Court sustained the Attorney General in holding that the combination violated the State anti-trust laws, even though it did not directly fix prices. So far as is known it is the first decision of any State court on the points involved and follows closely the decision of the United States Supreme Court last year in the hardwood lumber cases, according to Attorney General Barrett.

The association regulated the terms of credit and the general business practices of its members, and provided for cooperation that went far, but stopped short of the actual fixing of prices. Books were written describing the plan as "The New Competition," and it was believed, the Attorney General said, that the method would be proof against prosecution by the State or Federal authorities.

The Court in a sweeping decision declared that the Missouri statutes forbid not only agreements to lessen competition, but agreements which tend to lessen it. The decision said that the very creation of a system of machinery which can be improperly used is in itself a violation of the Missouri law.

The Court found that the increased cost of lumber was due to other factors as well, such as higher freight rates and greater labor costs, but condemned the lumber exchange in most positive terms.

(e) *New York Times*
New York, New York
September 1, 1923

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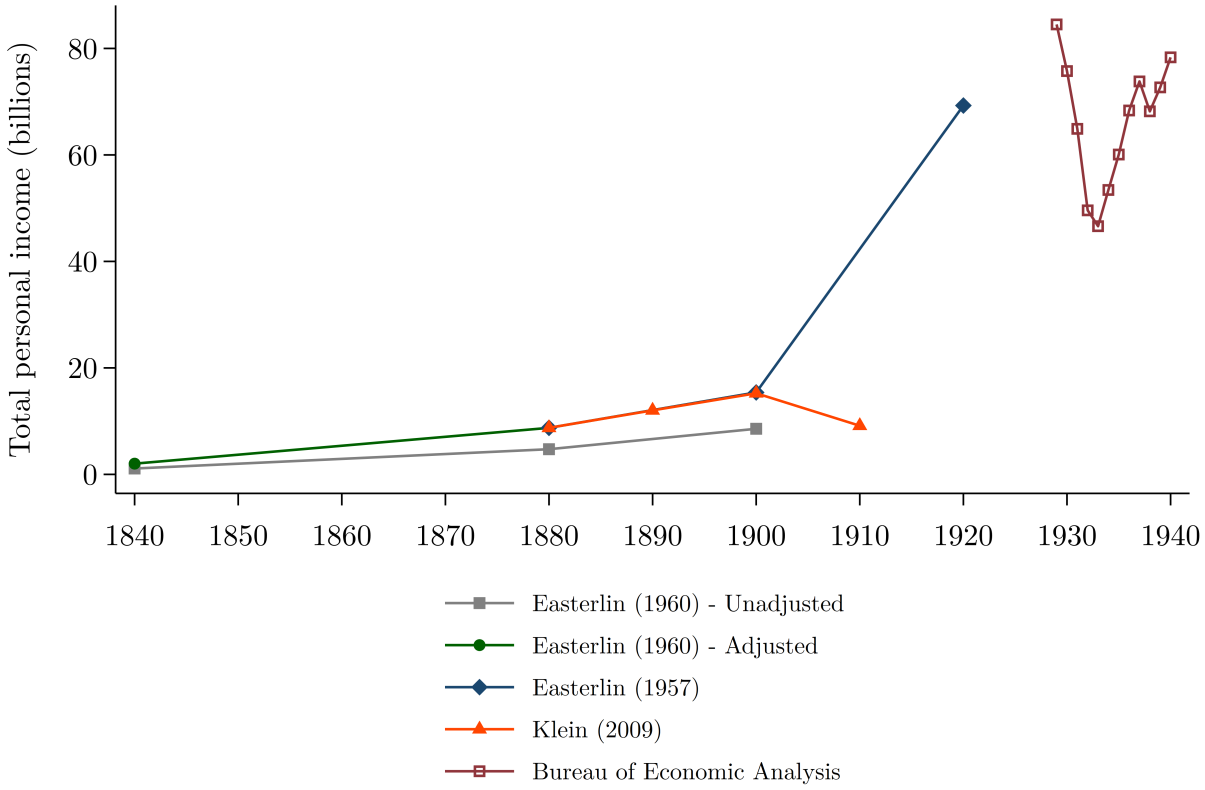
Figure A7: State-Level Personal Income Data Sources, 1840-1940



Notes: The composite series I construct on state-level personal income comes from four sources. Tables A-1, A-2, and A-3 in Easterlin (1960) provide data for 1840; Table 8 in Klein (2009) provides data for 1880 through 1910; Table Y-1 in Easterlin (1957, p. 753) provides data for 1920; and Bureau of Economic Analysis (2025) provides data for 1929 and later. The data Easterlin (1960) provides for 1840, 1880, and 1900 are adjusted upwards slightly to align with the estimates Easterlin (1957) and Klein (2009) provide for 1880 through 1900.

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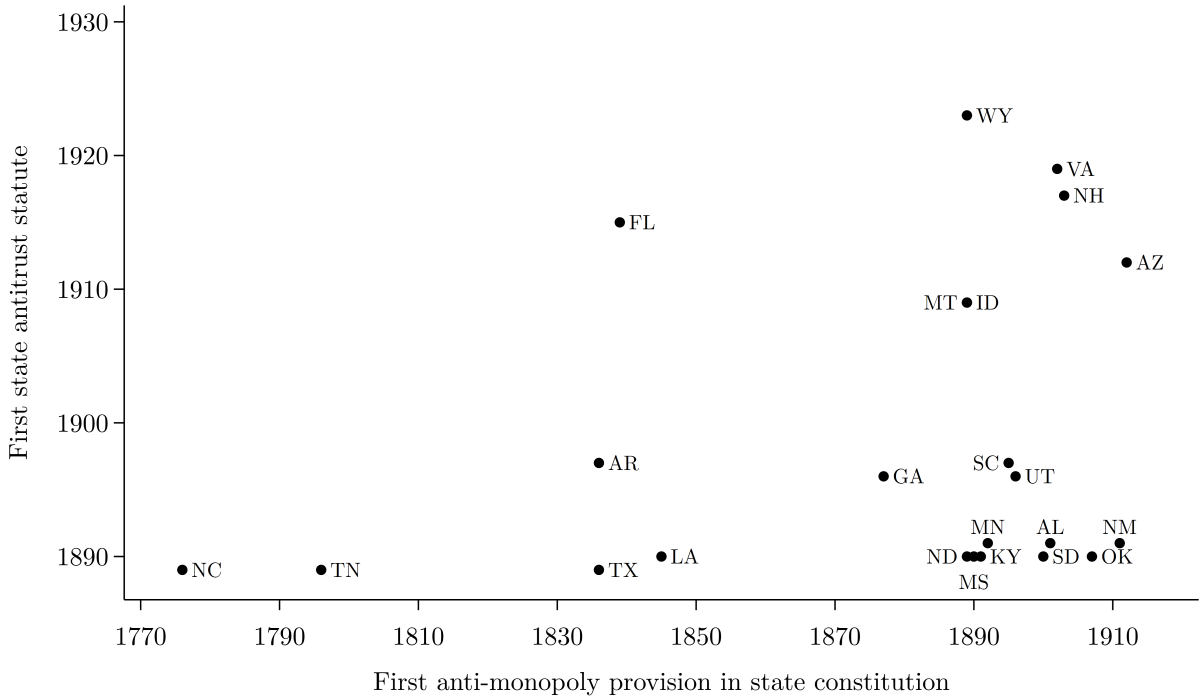
Figure A8: Adoption of State Antitrust Statutes and Adoption of Anti-Monopoly Provisions in State Constitutions, 1776-1930



Notes: This figure illustrates the relationship between adoption of an antitrust statute and adoption of an anti-monopoly constitutional provision, among states that adopted both of these measures.

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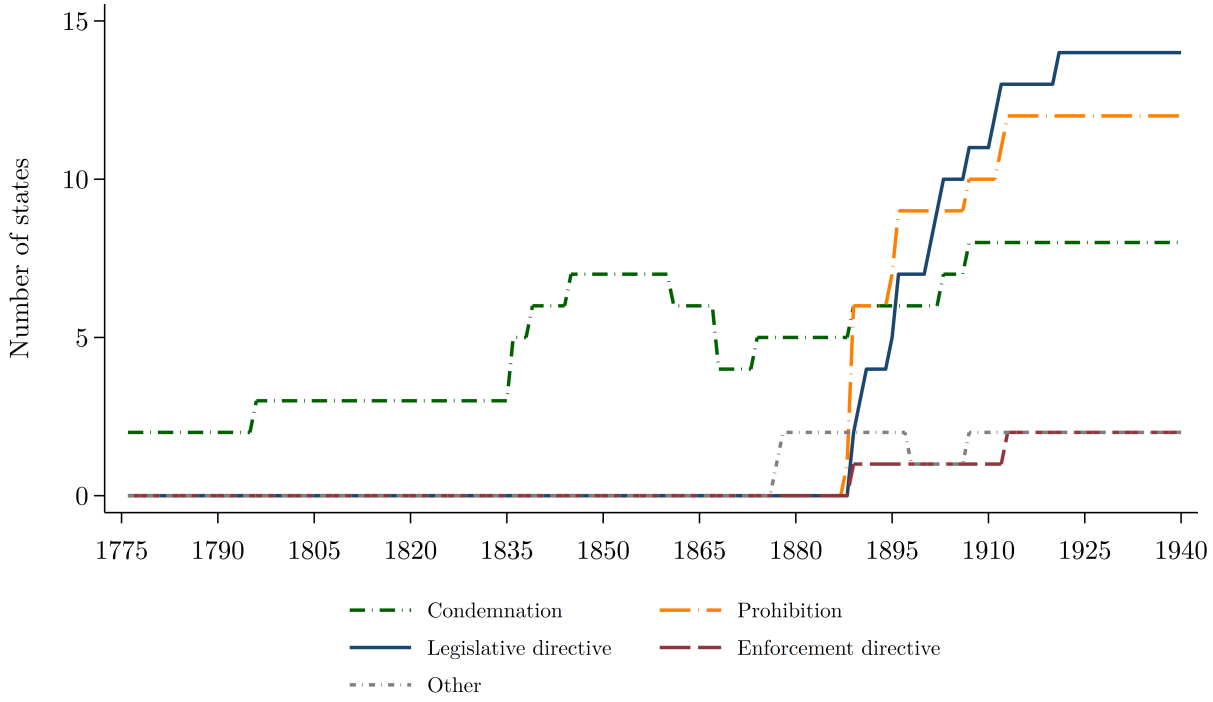
Figure A9: Contents of Anti-Monopoly Provisions in State Constitutions, 1776–1940



Notes: This figure illustrates the number of states with various kinds of anti-monopoly provisions in their state constitutions between 1776 and 1940. See Table A2 for definitions of these classes of provisions.

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Table A2: Contents of Anti-Monopoly Provisions in State Constitutions

Type	**Definition**
Condemnation	Decries the existence of monopolies.
Prohibition	Declares that combinations, trusts, and/or other restraints of trade are illegal.
Legislative directive	Orders the state's legislature to pass laws to regulate monopolies.
Enforcement directive	Designates a state officer, such as the Attorney General, to enforce the constitutional provision against monopolies.
Other	Deals with competition policy in a manner not covered by the above categories.

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Table A3: Descriptive Statistics for Ever- and Never-Treated States, 1880

	(1) Ever-Treated	(2) Never-Treated	(3) Difference
Population (thousands)	1192.75	767.10	425.657
	(1038.23)	(1348.72)	(410.928)
Urban share of population	0.216	0.301	-0.086
	(0.166)	(0.236)	(0.067)
Foreign-born share of population	0.151	0.168	-0.016
	(0.120)	(0.117)	(0.045)
Agricultural revenue per capita	42.80	43.66	-0.859
	(16.83)	(19.77)	(6.489)
School expenditures per capita	1.768	1.935	-0.167
	(1.153)	(0.973)	(0.418)
Railroad miles per 1,000 square miles	57.33	77.70	-20.371
	(62.07)	(70.45)	(23.745)
Average value of farmland (millions)	18.39	26.00	-7.611
	(15.36)	(15.99)	(5.768)
Newspapers per 10,000 population	2.43	2.83	-0.393
	(1.19)	(1.53)	(0.471)
Personal income per capita	187.87	241.61	-53.737
	(115.03)	(142.49)	(44.951)
Grange chapters per 1,000,000 population (1887)	591.47	398.73	192.745
	(369.20)	(448.36)	(145.280)
Greenback vote share (1884)	0.033	0.011	0.021
	(0.030)	(0.009)	(0.018)
Share of manufacturing employment in industries with a trust by 1904	0.785	0.727	0.058
	(0.169)	(0.200)	(0.066)
Observations	36	9	45

Notes: Columns (1) and (2) report means and standard deviations for ever-treated and never-treated states, respectively. Column (3) shows the difference in means, with standard errors in parentheses. Some variables are only consistently available at the state level in 1880, so each observation represents a single state-level mean; the sample size thus differs from that in the regression analysis, which uses disaggregated data. The source for the first eight measures is the National Historical Geographic Information System (Manson et al. 2022). State-level income estimates are from Easterlin 1960. Grange chapter data are courtesy of the National Grange. Greenback vote shares are from Inter-University Consortium for Political and Social Research (1999). I computed the last measure using data from Moody (1904) and the 1880 census of manufactures. Oklahoma, North Dakota, and South Dakota are excluded due to data limitations. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

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Table A4: Descriptive Statistics for Early- and Late-Treated States, 1880

	(1) Early-Treated	(2) Late-Treated	(3) Difference
Population (thousands)	1523.36	862.14	661.220*
	(1108.22)	(871.57)	(332.315)
Urban share of population	0.176	0.255	-0.078
	(0.130)	(0.191)	(0.054)
Foreign-born share of population	0.125	0.177	-0.052
	(0.107)	(0.130)	(0.040)
Agricultural revenue per capita	48.88	36.73	12.147**
	(15.85)	(15.92)	(5.296)
School expenditures per capita	1.597	1.940	-0.342
	(1.111)	(1.200)	(0.385)
Railroad miles per 1,000 square miles	47.39	67.28	-19.889
	(36.24)	(80.06)	(20.714)
Average value of farmland (millions)	15.78	21.00	-5.222
	(11.47)	(18.43)	(5.116)
Newspapers per 10,000 population	2.217	2.649	-0.432
	(1.016)	(1.344)	(0.397)
Personal income per capita	139.10	236.64	-97.533**
	(53.00)	(139.28)	(35.124)
Grange chapters per 1,000,000 population (1887)	725.13	449.46	275.669**
	(402.58)	(275.96)	(120.911)
Greenback vote share (1884)	0.034	0.031	0.003
	(0.033)	(0.028)	(0.018)
Share of manufacturing employment in industries with a trust by 1904	0.766	0.806	-0.041
	(0.188)	(0.150)	(0.058)
Observations	18	18	36

Notes: Columns (1) and (2) report means and standard deviations for "early-treated" states, which adopted a statute before 1895, and "late-treated" states, which adopted a statute in 1895 or later, respectively. Column (3) shows the difference in means, with standard errors in parentheses. Some variables are only consistently available at the state level in 1880, so each observation represents a single state-level mean; the sample size thus differs from that in the regression analysis, which uses disaggregated data. The source for the first eight measures is the National Historical Geographic Information System (Manson et al. 2022). State-level income estimates are from Easterlin 1960. Grange chapter data are courtesy of the National Grange. Greenback vote shares are from Inter-University Consortium for Political and Social Research (1999). I computed the last measure using data from Moody (1904) and the 1880 census of manufactures. Oklahoma, North Dakota, and South Dakota are excluded due to data limitations. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

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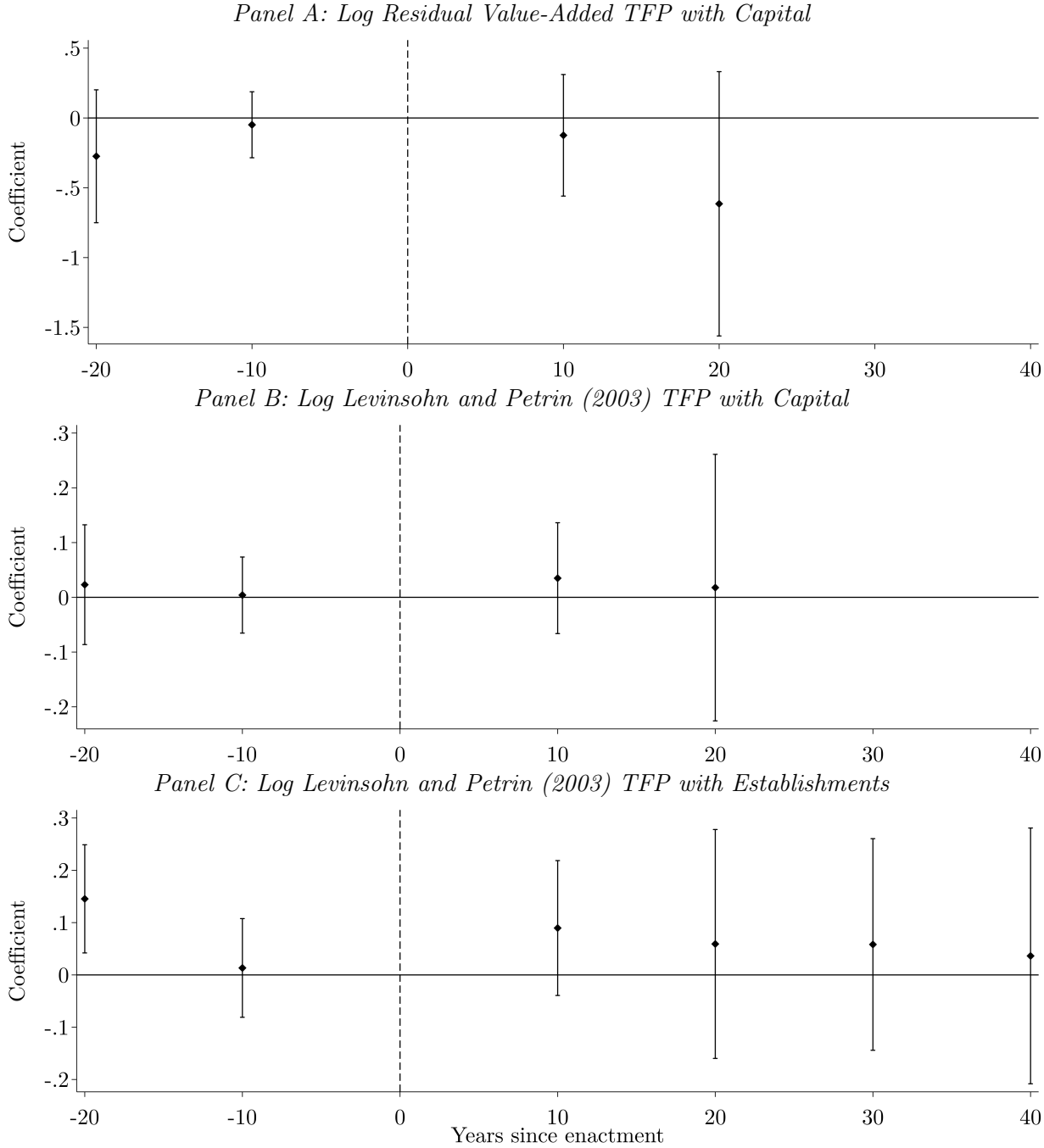
Figure A10: Event Study Results for Productivity Outcomes



Notes: Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included. TFP is A) the residual from a Cobb–Douglas production function; B) the first-stage residual from a Levinsohn and Petrin (2003) specification where labor is the free variable, materials is the unobserved productivity proxy, and capital is the state variable; and C) the same LP residual but using establishments as the state variable due to the limited availability of capital.

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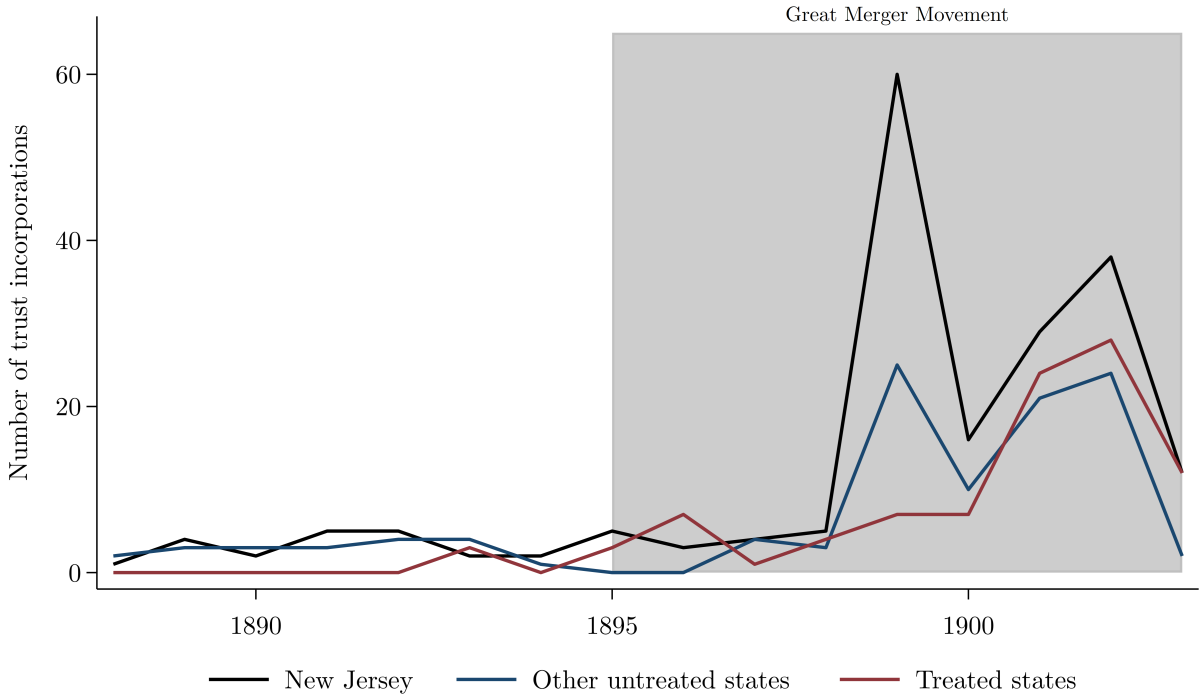
Figure A11: Trust Incorporations by Treatment Status, 1888-1903



Notes: This figure illustrates the number of trusts incorporating over time, by the contemporaneous treatment status of the state where they chose to incorporate. Data are from Moody (1904).

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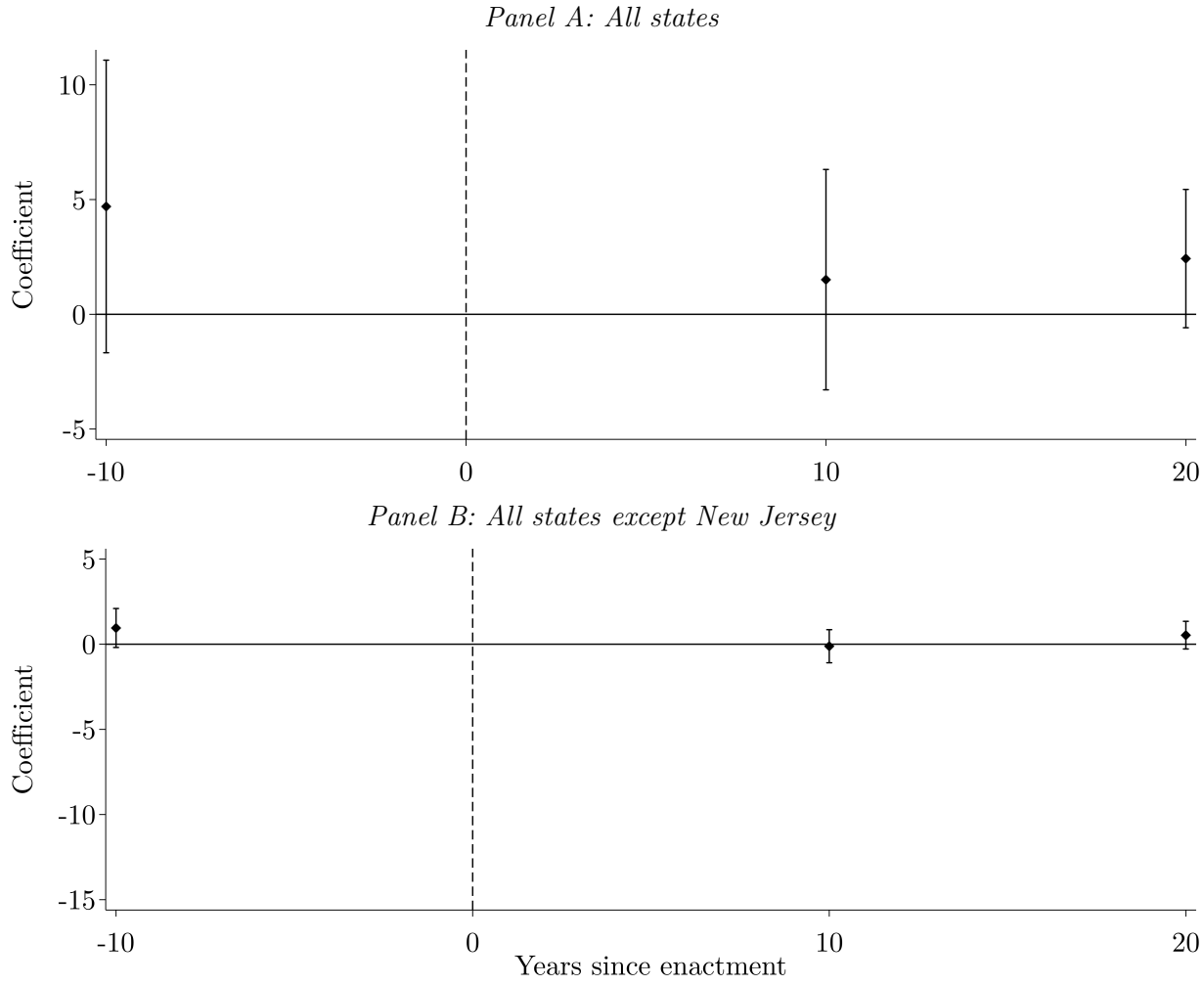
Figure A12: Effect of Enactment on Number of Trust Incorporations



Notes: The dependent variable is the number of trust incorporations by state and decade. Data are from Moody (1904). Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state.

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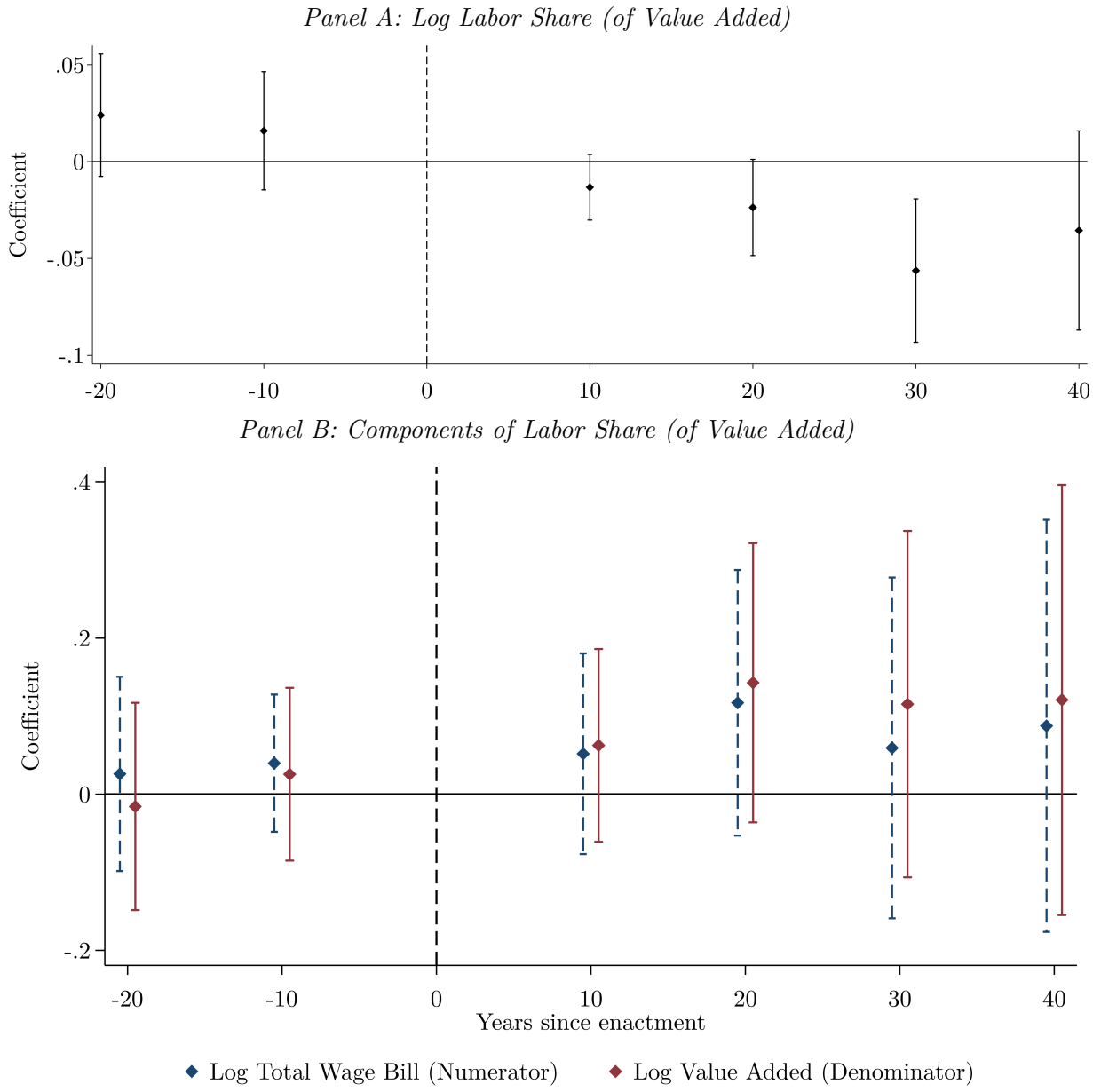
Figure A13: Event Study Results for Labor Share of Value Added and Components



Notes: Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

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B Methodology for Identifying and Coding State Antitrust Laws

To identify all state antitrust statutes in effect between 1860 and 1940, I developed a list of common words and phrases appearing in state antitrust statutes other scholars had already identified.[57] Then, I used HeinOnline, a popular online database for legal research, to search state session laws and state legal codes for these terms. The specific search terms I used are `anti-trust`, `antitrust`, `unfair trade practices`, `restraint of trade`, `pools`, `pooling`, `monopoly`, `monopolies`, `conspiracy against trade`, `combination`, `price fixing`, `fix the price`, `substantially lessen competition`, and `unreasonably restrain trade`. I used the Boolean operator "or" in my searches to ensure that session laws and historical codes containing *any* of these terms would be returned. Despite returning many false positives, which I tossed out upon inspection, this approach allowed me to cast a wide net and reduce the chance of missing any relevant laws.

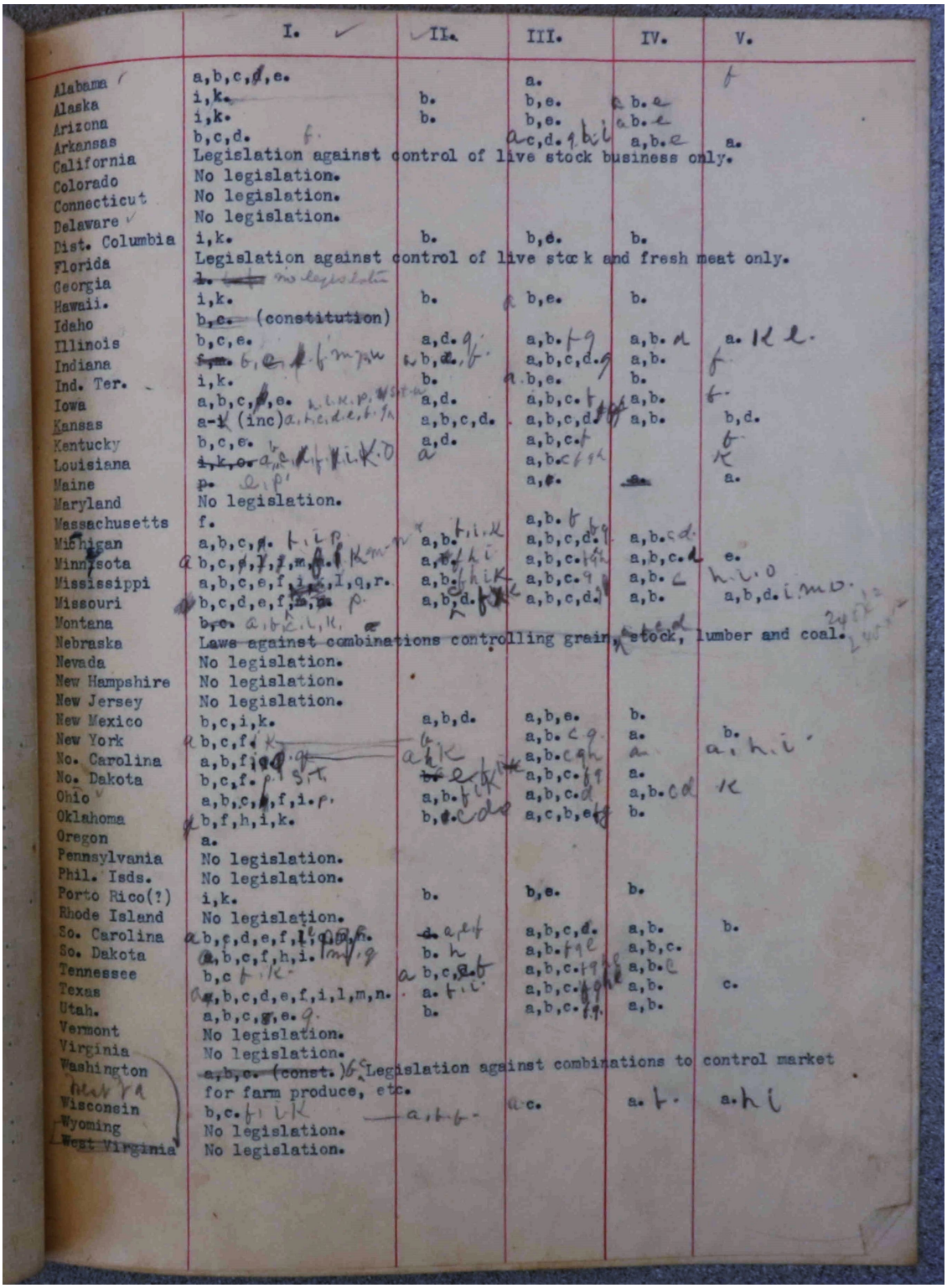
I considered any state statute outlawing restraints against trade, monopolization, horizontal price fixing, horizontal output restrictions, trustee control of corporations, anticompetitve stock purchases, refusals to deal, or some combination thereof to be a state antitrust statute. Table B1 defines each of these acts. Price discrimination, tying, predatory pricing, and retail price maintenance are notably excluded from the list of infractions I counted. In my review, I found that state statutes prohibiting these forms of anticompetitive conduct were often enacted independently of a general antitrust statute prohibiting more basic forms of anticompetitive conduct, such as price fixing. As a result, I excluded price discrimination, tying, predatory pricing, and retail price maintenance from my analysis to avoid characterizing, for example, a state that did not prohibit price fixing—but did prohibit price discrimination—as having an antitrust statute. Further, to avoid counting some sections of a chapter or title but not others, I regarded whole chapters or titles as antitrust statutes as long as they contained at least one eligible element (e.g., a prohibition of monopolization), even if they contained one or more sections with ineligible elements (e.g., a prohibition of price discrimination). I also did not consider statutes prohibiting price gouging or profiteering to be antitrust laws. Further, I did not count statutes clarifying that certain groups (e.g., labor unions or agricultural associations) did not constitute trusts. As described in Section 3, I do not regard statutes applying to a single industry, rather than to commerce more generally, as antitrust statutes in this paper. However, I do consider statutes that apply to "necessities of life" to be antitrust statutes given the breadth of this term.[58] I coded laws as in effect the year they were enacted, regardless of the exact enactment date. Further, in cases where a law was repealed or overturned, I coded the law as inactive in the year it was repealed or overturned.[59]

[57] Forrest (1896) and Halle (1899) are two early works that served as a helpful starting point.

[58] Connecticut's 1911 antitrust law is one example of a law containing this language. Act of August 15, 1911, Chapter 185, 1911 Connecticut Acts 1461.

[59] For example, if a law had been passed in 1900 and overturned in 1903, I would have coded the law as in effect in 1900, 1901, and 1902. In 1903, I would have again coded the law as inactive.

Figure B1: Bureau of Corporations Internal Memorandum on State Antitrust Laws



	I.	II.	III.	IV.	V.
Alabama	a,b,c,d,e.		a.		f
Alaska	i,k.	b.	b,e.	b.	
Arizona	i,k.	b.	b,e.	b.	
Arkansas	b,c,d.		a,c,d.	a,b.	a.
California	Legislation against control of live stock business only.				
Colorado	No legislation.				
Connecticut	No legislation.				
Delaware	No legislation.				
Dist. Columbia	i,k.	b.	b,e.	b.	
Florida	Legislation against control of live stock and fresh meat only.				
Georgia	~~b.~~ no legislation				
Hawaii.	i,k.	b.	b,e.	b.	
Idaho	b,c. (constitution)				
Illinois	b,c,e.	a,d.	a,b.	a,b.	a.
Indiana	~~f.~~ b,	b,	a,b,c,d.	a,b.	f
Ind. Ter.	i,k.	b.	a,b,e.	b.	
Iowa	a,b,c,~~d~~,e.	a,d.	a,b,c.	a,b.	f.
Kansas	~~a-k~~ (inc)	a,b,c,d.	a,b,c,d.	a,b.	b,d.
Kentucky	b,c,e.	a,d.	a,b,c.		f
Louisiana	~~i,k,o.~~	a	a,b.		k
Maine	~~p.~~		a,~~e.~~	~~a.~~	a.
Maryland	No legislation.				
Massachusetts	f.		a,b.		
Michigan	a,b,c,~~d~~.	a,b.	a,b,c,d.	a,b.c.	
Minnesota	b,c,~~d~~,~~f~~,~~g~~,~~h~~.	a,~~b~~.	a,b,c.	a,b,c.	e.
Mississippi	a,b,c,e,f,~~i~~,~~k~~,l,q,r.	a,b.	a,b,c.	a,b.	h,i,o
Missouri	b,c,d,e,f,~~m~~,~~n~~.	a,b,d.	a,b,c,d.	a,b.	a,b,d,i,m,o.
Montana	~~b,c.~~				
Nebraska	Laws against combinations controlling grain, stock, lumber and coal.				
Nevada	No legislation.				
New Hampshire	No legislation.				
New Jersey	No legislation.				
New Mexico	b,c,i,k.	a,b,d.	a,b,e.	b.	
New York	b,c,f,~~k~~.		a,b.	a.	b.
No. Carolina	a,b,f,~~g~~.		a,b.	a.	a,h,i.
No. Dakota	b,c,f.		a,b,c.	a.	
Ohio	a,b,c,~~d~~,f,i.	a,b.	a,b,c,d.	a,b.	k
Oklahoma	b,f,h,i,k.	b,	a,c,b,	b.	
Oregon	a.				
Pennsylvania	No legislation.				
Phil. Isds.	No legislation.				
Porto Rico(?)	i,k.	b.	b,e.	b.	
Rhode Island	No legislation.				
So. Carolina	a,b,c,d,e,f,~~i~~,~~q~~,~~r~~.	~~d.~~	a,b,c,d.	a,b.	b.
So. Dakota	a,b,c,f,h,i.	b. h	a,b.	a,b,c.	
Tennessee	b,c	a b,c,	a,b,c.	a,b.	
Texas	a,b,c,d,e,f,i,l,m,n.	a.	a,b,c.	a,b.	c.
Utah.	a,b,c,~~d~~,e.	b.	a,b,c.	a,b.	
Vermont	No legislation.				
Virginia	No legislation.				
Washington	~~a,b,c.~~ (const.) b. Legislation against combinations to control market for farm produce, etc.				
Wisconsin	b,c.	a,b.	a,c.	a.	a,h,i
Wyoming	No legislation.				
West Virginia	No legislation.				

Notes: This photograph depicts a page from a 1903 internal memorandum on state antitrust laws authored by staff at the Bureau of Corporations. Each column represents some aspect of the state's antitrust law. For example, column I tallies the acts declared illegal under each state's antitrust law, where each letter represents a different illegal act and the meaning of each letter is defined elsewhere in the document. I took this photograph during a visit to the National Archives.

Table B1: Illegal Acts Under State Antitrust Laws

Illegal Act	Definition
Restraint of trade	To restrict competition or restrain trade in the production, manufacture, or sale of an article or commodity.
Monopolization	To monopolize or to attempt to monopolize the production, manufacture, or sale of an article or commodity.
Horizontal price fixing	To form a combination to control the price of an article or commodity.
Horizontal output restriction	To form a combination to limit the quantity of any article or commodity to be produced, manufactured, or sold.
Trustee control of corporations	To sell trust certificates establishing trustee control of several firms that would otherwise compete.
Anticompetitive stock purchases	When one firm buys shares or stock in another firm for the purpose of restricting competition.
Refusal to deal	To refuse to sell because purchaser is not a combination member or to refuse to deal with customers or suppliers who transact with a competitor.

I also compared my database to other scholars' compilations of state antitrust laws to ensure accuracy. For example, Forrest (1896), Halle (1899), Bureau of Corporations (1915), and Works Progress Administration (1940) are contemporaneous works on state and federal antitrust laws. Any time I encountered an accounting of state antitrust statutes that disagreed with my own, I reviewed the differences in detail to confirm my understanding was correct. I also compared my database to internal memoranda on state antitrust laws from the now-defunct Bureau of Corporations. Figure B1 provides an example of one such memorandum. These documents were particularly helpful because they illustrated how contemporaneous regulators at the Bureau of Corporations viewed state antitrust law. I obtained copies of these documents during a visit to the National Archives in August 2022.

After identifying state antitrust statutes in effect between 1860 and 1940, I analyzed the content of these laws. For example, I coded damages authorized by state antitrust statutes as actual damages, double damages, or treble damages. In cases where the damages plaintiffs could seek differed according to the antitrust violation, I coded the larger allowable damages.[60] I also only

[60]For example, a 1907 amendment to Indiana's antitrust statute allowed treble damages to be sought for injury due to restraint of trade, horizontal price fixing, horizontal output restriction, or monopolization. Meanwhile, an 1899 Indiana law continued to set maximum damages equal to the "costs [accrued to injured persons] and a reasonable attorney's fee" for injury due to refusals to deal. In 1907 and later, I code the damages authorized by Indiana's antitrust statute as treble damages, even though the 1899 statute continued to set damages for refusals to deal at a lower amount. Act of March 3, 1899, Chapter 148, §4, 1899 Indiana Acts 257. Act of March 11, 1907, Chapter 243, §7, 1907 Indiana Acts 490.

coded statutes as declaring antitrust violations to be torts when the statute empowered individual people or corporations harmed by anticompetitive conduct to sue under the antitrust statute. If the statute, for example, only empowered the state's Attorney General to sue, I did not count the statute as declaring antitrust violations to be torts. One special case comes from Alabama's 1919 antitrust law, which allowed "such damages [to] be recovered as the jury [saw] fit to assess."[61] This language suggests damages could theoretically attain any value. However, since I encountered no other instances of states allowing effectively unlimited damages, I coded this statute as allowing the next-highest level of damages I coded (treble damages).

Coding the criminal penalties described in state antitrust statutes was mostly straightforward, but some rules I applied are as follows. In cases where a law described one set of minimum and maximum fines for an offender's first violation, and a different set of minimum and maximum fines for an offender's second violation, I counted the first set of fines in my coding. Further, though some states set different minimum and maximum fines for corporate offenders and individual offenders, I did not distinguish between the two fine types in my coding. Instead, I coded minima as $\min\{\texttt{minimum fine for corporations}, \texttt{minimum fine for individuals}\}$ and maxima as $\max\{\texttt{maximum fine for corporations}, \texttt{maximum fine for individuals}\}$.[62] Similarly, in cases where fines differed according to the antitrust violation, I determined minima and maxima across all violations.[63] Since I counted prison sentences in months, I coded 30-day minimum prison sentences as one month and one-year minimum prison sentences as 12 months. I also coded 24-hour minimum prison sentences as $1/30$ or 0.0333 months.[64]

[61] Act of September 30, 1919, Chapter 741, 1919 Alabama Acts 1088.

[62] One exception to this rule comes from an 1890 Iowa law. In this case, I coded the individual fines rather than the corporate fines because the statute sets corporate fines as between 1 percent and 20 percent of the firm's capital stock. Whether this amounted to more or less than the individual fine would have depended on the firm. Act of May 6, 1890, Chapter 28, 1890 Iowa Acts 41.

[63] For example, an 1897 Indiana law established a minimum fine of $100 for price fixing, while an 1899 Indiana law established a minimum fine of $50 for refusing to deal. I thus coded Indiana's minimum fine for antitrust violations as $100 in 1897 and 1898 and $50 in 1899 and the years thereafter (until 1907, when the Indiana General Assembly adopted a wholly new antitrust statute). Act of March 5, 1897, Chapter 104, 1897 Indiana Acts 159. Act of March 3, 1899, Chapter 148, 1899 Indiana Acts 257. Act of March 11, 1907, Chapter 243, 1907 Indiana Acts 490.

[64] For example, Montana's 1909 antitrust law describes a 24-hour minimum prison sentence. Act of March 6, 1909, Chapter 97, 1909 Montana Acts 127.

C Food Retail Price Analysis

Although not the central focus of the paper, this appendix presents a supplementary analysis of how state antitrust laws affected retail food prices. To facilitate this analysis, I digitized and assembled a panel of data on the prices consumers paid at retail stores for various foods in cities across the United States. The data span 1851 to 1911, with some gaps, and are drawn from Weeks (1886) and three subsequent reports published by the Department of Commerce and Labor.[65] The foods covered in this dataset (see Figure C1) reflect the consumption patterns of typical households during the study period and serve as a meaningful proxy for the cost of living in the late nineteenth and early twentieth centuries; Weeks (1886, p. 1) aptly describes them as "the chief necessaries of life."

To estimate the average effect of state antitrust law enactment on food prices, I use a stacked difference-in-differences design that accommodates variation in treatment timing across states.[66] For each of the 30 food items, I construct a separate dataset that stacks treated states around their year of adoption and includes both never-treated and not-yet-treated states as controls. The estimating equation is:

$$\ln(P_{ast}) = \beta_0 + \beta_1(statelaw_s \times post_{st}) + \Gamma X_{st} + \delta_{as} + \delta_{at} + \varepsilon_{ast} \tag{5}$$

where s denotes a state, t denotes a year, and a denotes a stack. The log retail price of a given food item is denoted by $\ln(P_{st})$, whether state s adopted an antitrust law is indicated by $statelaw_s$, and the time period after which a state antitrust law was adopted is denoted by $post_{st}$. The model also includes state fixed effects δ_s and year fixed effects δ_t, as well as a vector of time-varying controls X_{st}. Specifically, X_{st} includes state-level personal income (to account for changes in consumer purchasing power), and county-level population (to capture demand and urbanization), median occupational score (a proxy for local economic development), and the share of employment in agriculture (to account for variation in food production and consumption patterns). Standard errors are clustered at the state level. The coefficient β_1 captures the average treatment effect on the treated for each food item, netting out time-invariant state characteristics, national trends, and covariates.

The estimated effects of state antitrust laws on retail food prices are mixed overall, but the results suggest meaningful price declines for meat products and point to a substantial reduction in the cost of overall household expenditures on food. Of the 30 foods shown in Figure C1, six exhibit statistically significant effects at the 95 percent confidence level. Four of these six—bacon, fresh beef roast, fresh lamb or mutton, and fresh veal—show negative and significant coefficients, suggesting that the enactment of state antitrust laws was associated with lower prices for these products. Notably, all four are meat products. Two additional meats—fresh beef steak and fresh chicken—

[65] The sources for this dataset are Weeks (1886) and Department of Commerce and Labor (1904, 1906, 1912). These reports provide annual price data for the years 1851–1880, 1890–1903, 1904–1905, and 1907–1911, respectively. I digitized the latter three reports myself and thank Michael Haines for sharing digitized data from Weeks (1886).

[66] See Section 4 for more information about the stacked difference-in-differences design I employ.

also have negative coefficients that fall just short of statistical significance. Taken together, these results suggest that state antitrust laws may have been effective in reducing the prices of meat. This effect is plausibly linked to the intense public and political scrutiny of the Chicago meat packers in the late nineteenth century, which may have placed pressure on the leading firms to scale back price-fixing and other anticompetitive practices in response to the enactment of state antitrust laws.[67] Consistent with this pattern, a "representative food basket"—i.e., a weighted index of food retail prices based on household expenditure shares circa 1890, as documented by Haines (1989)—became approximately 27 percent cheaper following the enactment of a state antitrust law. While most individual food price effects are imprecisely estimated, the index-level result points to a potentially broad decline in consumer food spending.

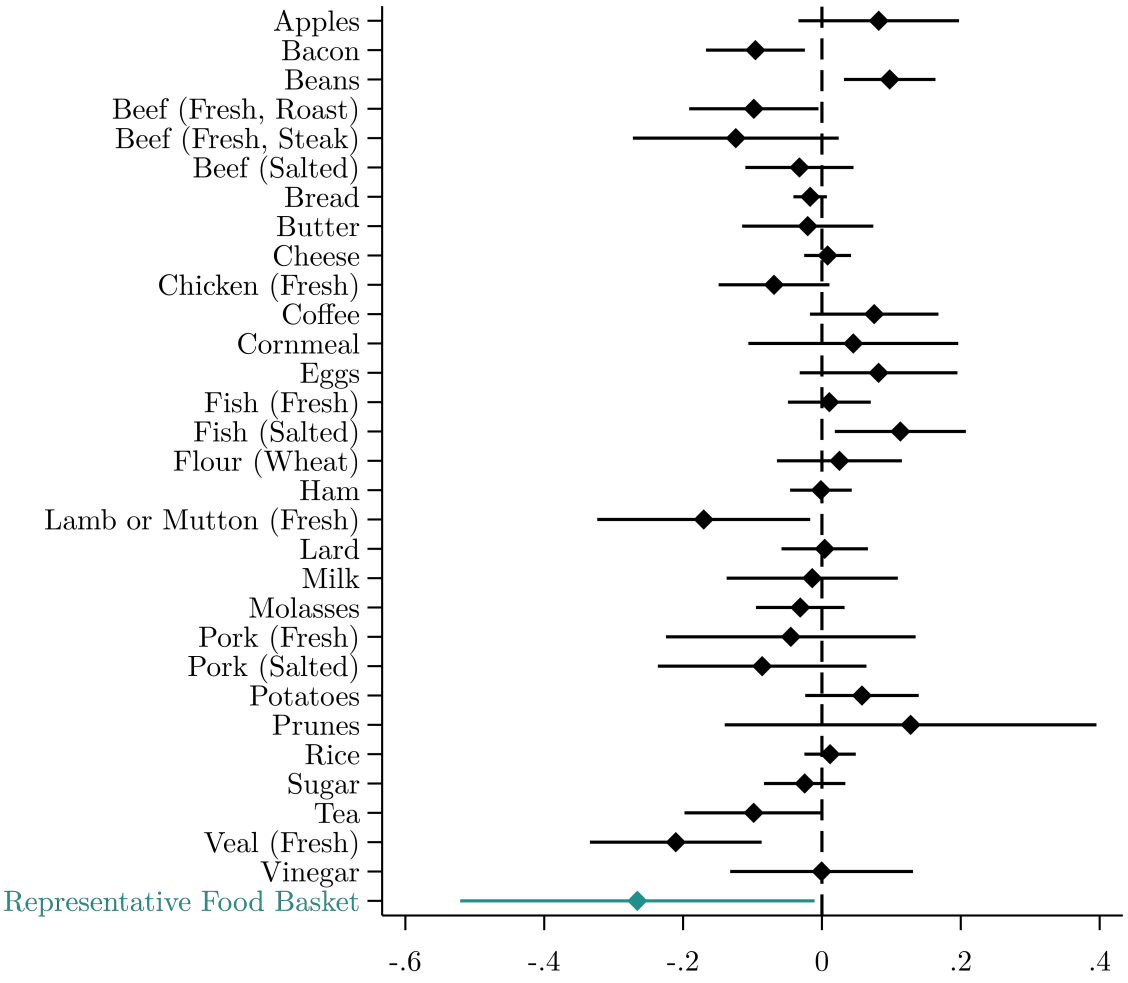
These results should be interpreted with some caution. As discussed in Section 2.3, the impact of an antitrust law depends in part on pre-existing market conditions. If the market for a given food was already competitive before the enactment of a state antitrust law, substantial price effects would not be expected. Accordingly, the many null results in Figure C1 is in line with the view that antitrust laws had limited scope to reduce prices in already competitive markets. Two results—positive coefficients for beans and for salted fish—run counter to this pattern, though a small number of counterintuitive results may arise by chance given the number of outcomes tested. On the other hand, laws introduced in markets that were initially cartelized or otherwise uncompetitive are more likely to generate price reductions through increased competition. One notable case is sugar, a product from a notoriously concentrated industry. The Sugar Trust, formed in 1887, controlled a dominant share of national refining capacity and was a frequent target of criticism and regulatory scrutiny.[68] Despite its notoriety, no state successfully sued the Sugar Trust under their antitrust statute.[69] Although the coefficient for sugar is negative, it is not statistically significant, offering a cautionary example of a highly concentrated industry where antitrust law may not have translated into measurable benefits for consumers—perhaps due to a lack of enforcement.

[67] In the late nineteenth century, meatpacking was dominated by a few large Chicago-based firms. By 1890, the four largest—Armour, Swift, Morris, and Hammond—accounted for about 89 percent of the national supply of dressed beef (Yeager 1981, p. 67). Known collectively as the "Beef Trust," these firms processed a wide range of meats, not just beef, and were often accused of price-fixing, market division, and collusion with railroads. Public outrage over the Beef Trust made meatpacking a target of early state antitrust enforcement efforts. For example, in 1903, the Missouri Supreme Court found five major meat packers guilty of violating the state's antitrust statute and ordered them to cease operations in the state, pay $5,000 each in fines, and cover court costs. *State ex rel. Crow v. Armour Packing Co.*, 173 Mo. 356, 73 S.W. 645 (1903). Arkansas also brought several suits of a similar nature against the Beef Trust, including a notable case against Hammond that the U.S. Supreme Court decided in the state's favor. *Hammond Packing Company v. State of Arkansas*, 212 U.S. 322 (1909). See Willis (2021) for a detailed account of Arkansas's antitrust enforcement efforts during the Progressive Era. State-level enforcement actions, such as those by Missouri and Arkansas, may help explain the strong price effects observed for meat products.

[68] Henry Osborne Havemeyer, founder of the Sugar Trust, testified that 17 or 18 of 21 refineries belonged to the Trust, with the remainder accounting for about 30 percent of the national market (Industrial Commission 1900).

[69] New York did famously dissolve a sugar refining company for joining the Sugar Trust. This action, upheld in *People v. North River Sugar Refining Company*, was brought as a *quo warranto* proceeding under corporate law rather than using antitrust law. 121 N.Y. 582 (1890).

Figure C1: Difference-in-Differences Results for Food Retail Prices



Notes: Each coefficient in this figure represents a separate estimation of equation 5, where the dependent variable is the log retail price of the listed good. Estimated values of β_1 from equation 5 are shown, and bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. Estimates are obtained using the stacked difference-in-differences method. The "representative food basket" refers to a price index constructed using the items consumed by a typical household around 1890, as documented by Haines (1989), where each food item is weighted by its share of household food expenditures. States whose antitrust laws were overturned or repealed are excluded. Controls capture state-level personal income and the county-level population, median occupational score, and share of employment in agriculture.